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No. 2

Dr. J. H. JEANS, PRESIDENT, in the Chair.

Redmond Barton Cafferata, Villa Natacha, Bvd. Carnot, Nice,
A.M., France ;
Henry William Mountcastle, Ph.D., Professor of Physics and
Astronomy, Western Reserve University, Cleveland, Ohio,
U.S.A. ; and
S. Lynn Rhorer, 26 Luckie Street, Atlanta, Georgia, U.S.A.,
were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of
the Society, the names of the proposers from personal knowledge being
appended :—

Instructor Lieut.-Commander Cecil William Baldwin, M.A., R.N.,
H.M.S. *Repulse* (proposed by M. A. Ainslie) ;
Robert Ballard, Milverton, Market Harborough (proposed by T. G.
Hirst) ;
Bernhard Richard Baumgardt, 626 West 30th Street, Los Angeles,
California, U.S.A. (proposed by F. C. Leonard) ;
George Edward Kevin Blythe, 110 King Edward's Road, Swansea
(proposed by S. Brodetsky) ;
Henri Brändli, Royal Irrigation Department, Bangkok, Siam
(proposed by F. W. Dyson) ;
Alfred John Gould, Wh.Sc., B.Sc., A.M.I.N.A., Assistant Professor
of Engineering in the University of Rangoon, Prome Court,
Rangoon, Burmah (proposed by L. M. Milne-Thomson) ;

- Malvalli Gundappa Krishna Murti, A.C.G.I., F.G.S., M.R.San.I., City and Guilds Engineering College, South Kensington, London, S.W. 7 (proposed by S. Chapman);
- John Ellis Owen, Llysfa, Abersoch, Carnarvonshire (proposed by W. H. Coombs);
- William Harold Robb, 84 McIlwraith Street, Prince's Hill, Melbourne, Victoria, Australia (proposed by John Stewart);
- Richard Alfred Rossiter, Detroit Observatory, Ann Arbor, Michigan, U.S.A. (proposed by W. J. Hussey);
- Gregory A. Shajn, Pulkovo Observatory (Southern Branch), Simeis, Crimea, Russia (proposed by J. Jackson);
- Kwanji Suda, Imperial Marine Observatory, Kobe, Japan; and 69 Grantchester Meadows, Cambridge (proposed by Harold Jeffreys);
- Rev. Francis Anthony Tondorf, S.J., Georgetown University, Seismological Observatory, West Washington, D.C., U.S.A. (proposed by E. C. Phillips); and
- Fleming Voltelin Van der Byl, F.R.G.S., F.R.Met.Soc., Bilton Grange School, near Rugby (proposed by B. Bonniksen).

Fifty-three presents were announced as having been received since the last meeting, including, among others:—

A. S. Eddington, *The Internal Constitution of the Stars*.

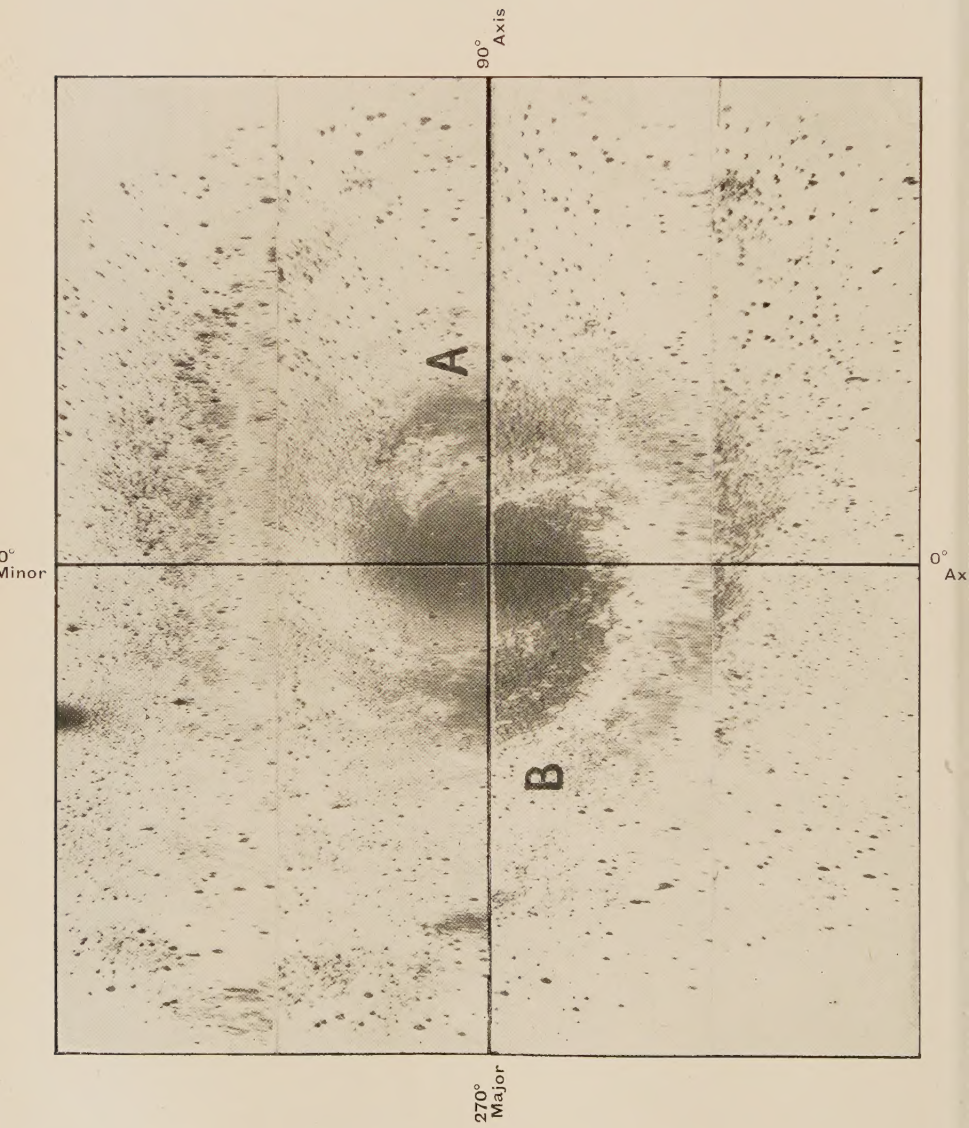
G. E. Hale, *Beyond the Milky Way*.

Scientific Papers of the 3rd Earl of Rosse, collected and republished by the Hon. Sir Charles Parsons (presented by Sir Charles Parsons). Rome, Specola Vaticana, *Astrographic Catalogue*. Appendix III., *Catalogue of Double Stars*.

Mr. Frank Robbins and Dr. L. J. Comrie were appointed auditors of the Treasurer's accounts for 1926.

The Spiral Form and Stellar Development of the Andromeda Nebula. By J. H. Reynolds. (Plates 4, 5.)

The important work on the Andromeda Nebula and M33 by Hubble during recent years has given us a definite indication of the distances and dimensions of these spirals. As the Andromeda Nebula is perhaps the nearest of these formations to show a definite spiral structure, it is best suited to a detailed examination of its development both in form and in stellar condensation. Owing to the considerable inclination of the spiral plane to the normal to the line of sight, the spiral development is difficult to follow in the photographs. A comparison of the major and minor axes of the ellipse show that this inclination is about 70° , on the assumption that the whole formation is roughly circular. If the two-dimensional photographic image is projected at the corresponding angle, the projection obtained should give an approximate idea of the real spiral form, as we know from edge-on examples of spirals that the third dimension need not be seriously considered





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in regions away from the nucleus. The lenticular nucleus, however, which in cases of developed spirals such as this is in the ratio of 4 to 1, begins to introduce a fictitious elongation in the projection along the minor axis at inclinations over 60° to the normal, which must be taken into account. The effect of this distortion is not serious until an angle of 85° is reached, and the composite photograph reproduced in Plate 4 needs only a comparatively small correction on this account along the minor axis of the nucleus, represented by the line $0^\circ-180^\circ$. It is, however, sufficient to mask the exact position of the two nodes in the nucleus from which the spiral arms emerge.

The nebula is of the massive type, the arms being of considerable breadth in marked contrast to the fine filamentous arms present in other examples such as N.G.C. 4254. It is developed through almost a complete revolution of the spiral, the stellar areas only appearing after half a revolution has been accomplished. The arm issuing in quadrant 90° to 180° (A) is considerably more irregular in form than that issuing in the opposite quadrant, and the stellar development is not so marked until it reaches the quadrant 180° to 270° . It is in this latter area that the star masses investigated by Hubble occur. In the arm B a considerable number of comparatively bright stars (15th and 16th magnitude) are shown in the quadrant 90° to 180° . The amorphous character of the nucleus and the surrounding regions is quite evident, in strong contrast to the outer stellar regions, and this is confirmed by star counts over the whole area of the nebula. The globular nebula N.G.C. 221 appearing in a distorted form at about p.a. 195° is probably in the same plane and connected with arm B, similar to the satellite nebula in the well-known spiral N.G.C. 3794/5. The radial velocity of N.G.C. 221 has been found by Slipher to be the same as that of the nucleus of the principal nebula (-300 km. per sec.), which is additional evidence of its intimate connection with the main nebula mass.

To ascertain the degree of stellar condensation over the whole area occupied by the nebula, star counts have been undertaken on Ritchey's photograph with a 24-inch reflector at Yerkes Observatory, stopped down to 18 inch. This is of excellent definition, showing stars down to 17th and probably to 18th magnitude. The regions dealt with by Hubble are too far from the optical axis to give satisfactory counts, so the extremities of the ellipse have been omitted. As the nebula lies in a galactic field, although 22° from the galactic equator, the number of comparatively bright stars which do not belong to the nebula is considerable. Counts were made in squares of 5 mm., the scale of the photograph being 1 mm. = $30''$ of arc. The average number of field stars in squares not occupied by the nebula is 7, and any number over 10, if appearing in several squares close together, may be taken as an indication of stellar condensation in the nebula. The lack of field stars in the nuclear region is, of course, owing to the long exposure of the bright nucleus and consequent uniform blackening of the film.

The second diagram shows two principal regions where the stars are evidently of nebular origin, viz. to the right of the minor axis between 90° and 180° , and in corresponding positions below the arm A between 270° to 90° (through 360°).

A detailed study of these regions with a large reflector would yield interesting results as to the development and magnitudes of the stars in these regions as they emerge from the amorphous nebulosity.

Counts made in the Hubble photograph of regions at the extremity of the ellipse yield an immense number of small star discs. The scale of the unenlarged photograph is about the same as the enlarged Yerkes image already dealt with, and similar squares give as many as 10 times the number, mostly of about the 20th magnitude. An interesting point is that the galactic non-nebular squares contain small star discs of this magnitude, which in all probability have no connection with the nebula.

On Dense Matter. By R. H. Fowler, F.R.S.

§ 1. *Introductory.*—The accepted density of matter in stars such as the companion of Sirius is of the order of 10^5 gm./c.c. This large density has already given rise to most interesting theoretical considerations, largely due to Eddington. We recognise now that matter can exist in such a dense state if it has sufficient *energy*, so that the electrons are not bound in their ordinary atomic orbits of atomic dimensions, but are in the main free—with sufficient energy to escape from any nucleus they may be near. The density of such “energetic” matter is then only limited *a priori* by the “sizes” of electrons and atomic nuclei. The “volumes” of these are perhaps 10^{-14} of the volume of the corresponding atoms, so that densities up to 10^{14} times that of terrestrial materials may not be impossible. Since the greatest stellar densities are of an altogether lower order of magnitude, the limitations imposed by the “sizes” of the nuclei and electrons can be ignored in discussions of stellar densities, and the structural particles of stellar matter can be treated as massive charged points.

Eddington has recently* pointed out a difficulty in the theory of such matter. Assuming it to behave more or less like a perfect gas, modified by its electrostatic forces and the sizes of such atomic structures as remain undissolved, there is a perfectly definite relation between the energy and the temperature, which depends on the density only to a minor degree. This assumption even here is not so unreasonable as appears at first sight. But even without it we naturally expect a perfectly definite relation between energy and temperature, in which there is a close correlation between large energies and large temperatures, small energies and small temperatures. The emission of energy by the star will proceed in the usual way at a rate depending on the surface temperature, and the internal temperatures must provide the gradient necessary to drive the radiation out. So long as the star contains matter at a high *temperature*, radiation of energy must presumably go on. But then, according to Eddington, there may come a time when a very curious state of affairs is set up. The stellar material will have

* Eddington, *The Internal Constitution of the Stars*, § 117, Cambridge Univ. Press (1926).

radiated so much energy that it has less energy than the same matter in normal atoms expanded at the absolute zero of temperature. If part of it were removed from the star, and the pressure taken off, what could it do?

The present note is devoted to a further consideration of this paradox. It is clear that the crucial point is the connection between the energy and the temperature. In a sense the temperature measures the "looseness" of the system, the number of possible configurations which it can assume, and therefore its radiation. These depend directly on the temperature, and only on the energy in so far as the energy determines the temperature. The excessive densities involved suggest that the most exact form of statistical mechanics must be used to discuss the relationship between the energy, temperature, and density of the material. This is a form suggested by the properties of atoms and the new quantum mechanics, which has been already applied to simple gases by Fermi and Dirac.* It may be accepted now as certain that classical statistical mechanics is not applicable at extreme densities, even to ideal material composed of extensionless mass-points, and that the form used here is fairly certainly the correct substitute. Its essential feature is a principle of exclusion which prevents two mass-points ever occupying exactly the same cell of extension h^3 of the six-dimensional phase-space of the mass-points. When this form of statistical mechanics is adopted, it at once appears that the suggested difficulty resolves itself, and there is really no difficulty at all. The apparent difficulty was due to the use of a wrong correlation between energy and temperature, suggested by classical statistical mechanics. When the correct relation is substituted, it is found that the limiting state of such dense stellar matter is one in which the *energy* is still, as it must be, excessively great, but the *temperature* is zero! Since the temperature determines the radiation, radiation stops when the dense matter has still ample energy to expand and form normal matter if the pressure happened to be removed. As the dense matter radiates its energy away, the number of its possible configurations rapidly falls, and therewith the temperature. The absolutely final state is one in which there is only one possible configuration left. Temperature then ceases to have any meaning, for the star is strictly analogous to one gigantic molecule in its lowest quantum state. We may call the temperature then zero.

Whether or no some such explanation may not be equally possible using other forms of statistical mechanics (perhaps the classical) I am not prepared to say. The new form used here seems for entirely independent reasons so satisfactory that its applicability need not be questioned. On application it clears up Eddington's question in a convincing manner, and I am content to leave the matter so.

§ 2. *The Equilibrium State of Dense Matter.*—It is obviously reasonable to consider dense stellar material as an assembly of free electrons and bare nuclei of net charge zero. It is, however, necessary to idealise the problem a little further and ignore the electrostatic charges, so that

* Fermi, *Rend. Acc. Lincei*, Ser. 6, **3**, 145 (1926); Dirac, *Proc. Roy. Soc., A*, **112**, 661 (1926). See also Fowler, *Proc. Roy. Soc., A*, **113**, 432 (1926), for a full account with other references.

the whole energy of the assembly is the kinetic energy of translation of the various particles. We shall later see reason to believe that this idealisation does not vitiate the results, or at least not their qualitative form, for we are really mainly concerned with calculations of amounts of phase-space, and these are independent of potential energy.

Consider a volume V of stellar material consisting of N/q bare nuclei of mass m_q and N free electrons of mass m , with total (kinetic) energy E . We will assume as a first approximation, as we have said, that the electrostatic forces may be ignored, and then that the statistics of assemblies of practically independent systems is applicable. Then* the total number of complexions C representing any state of the assembly is

$$C = \left(\frac{1}{2\pi i} \right)^3 \iiint \frac{dx}{x^{N+1}} \frac{dy}{y^{N/q+1}} \frac{dz}{z^{E+1}} \Pi_{\sigma}(1+xz^{\epsilon_{\sigma}})^2 \Pi_{\sigma}(1+yz^{\eta_{\sigma}}) \quad (1)$$

In this equation ϵ_{σ} ($\sigma = 1, 2, \dots$) are the possible energies of an independent electron in a volume V , and η_{σ} those of an independent nucleus. The power 2 enters because the electron has a structure (axis of spin) which gives it two possible orientations in a magnetic field, that is, a statistical weight 2. The nucleus probably also has a weight greater than unity, but the nuclear terms turn out to be unimportant, so that we need not spend time over this refinement. Students of classical statistical mechanics will recall that the classical formula differs from this only in having $\exp \{ \sum_{\sigma} 2xz^{\epsilon_{\sigma}} \}$ in place of $\Pi_{\sigma}(1+xz^{\epsilon_{\sigma}})^2$.

All the mean values associated with the equilibrium state of the assembly can be derived from C , and the value of C can be obtained by "steepest descents." Writing

$$G = G(x, z) = \sum_{\sigma} 2 \log(1+xz^{\epsilon_{\sigma}}), \quad G_q = G_q(y, z) = \sum_{\sigma} \log(1+yz^{\eta_{\sigma}}), \quad (2)$$

the critical value of the integrand is determined by the unique root λ, μ, θ of the equations

$$N = x \frac{\partial G}{\partial x}, \quad \frac{N}{q} = y \frac{\partial G_q}{\partial y}, \quad E = z \frac{\partial G}{\partial z} + z \frac{\partial G_q}{\partial z} \quad (3)$$

With these special values of x, y, z we have, among other equations,

$$N = \lambda \frac{\partial G}{\partial \lambda}, \quad E_e = \theta \frac{\partial G}{\partial \theta}, \quad (4)$$

where E_e is the average energy of the free electrons. Also

$$S = k \log C = k \left[G + G_q - N \log \lambda - \frac{N}{q} \log \mu - E \log \theta \right], \quad (5)$$

$$\theta = e^{-1/kT} \quad (6)$$

The number of possible energy values for an electron of mass m in a volume V which lie between τ and $\tau + d\tau$ is

$$\frac{2\pi V}{h^3} (2m)^{3/2} \tau^{1/2} d\tau \quad (7)^\dagger$$

* The following formulæ will be found in detail in Fowler, *loc. cit.*

† Dirac, *loc. cit.*, p. 671.

Therefore

$$G = \frac{4\pi(2m)^{\frac{3}{2}}}{h^3} V \Sigma \log(1 + \lambda \theta^\tau) \tau^{\frac{1}{2}} d\tau.$$

The values of ϵ_τ are extremely closely spaced, so that for a fixed value of θ we can replace the sum by an integral with negligible error and find

$$G = \frac{4\pi(2m)^{\frac{3}{2}}}{h^3} V \int_0^\infty \log(1 + \lambda \theta^\tau) \tau^{\frac{1}{2}} d\tau \quad (8)$$

Strictly we shall want to make $\theta \rightarrow 0$ ($T \rightarrow 0$). The form (8) might then fail, but as we shall really not be interested in the differences between $T = 0^\circ$, $T = 100^\circ$, or even $T = 1000^\circ$, this does not matter, and we shall assume the general validity of (8). The validity of some applications of (8) can be checked in the limit by direct calculation. N and E_ϵ are obtained from this by differentiation. Similar forms and equations hold for G_q , N/q , and E_q for the nuclei. The assembly is a thermodynamic system.*

Our chief interest is now to trace the series of equilibrium states of such an assembly as its energy is slowly radiated away. A loss of energy by radiation means necessarily a loss of entropy, and therefore a fall of temperature. We have therefore to trace the behaviour of the assembly as θ decreases—presumably as $\theta \rightarrow 0$. But by (4) and (8) N depends on a function of the form

$$\int_0^\infty \frac{\lambda \theta^\tau}{1 + \lambda \theta^\tau} \tau^{\frac{1}{2}} d\tau.$$

If λ has an upper bound b , this is less than

$$b \int_0^\infty \theta^\tau \tau^{\frac{1}{2}} d\tau = b \Gamma(\frac{3}{2}) / (\log 1/\theta)^{\frac{1}{2}},$$

and therefore tends to 0 as $\theta \rightarrow 0$. This would make $N \rightarrow 0$, which is impossible, as the number of particles is fixed. Since λ cannot have an upper bound, it is reasonable to assume that $\lambda \rightarrow \infty$. We shall see shortly that this is a necessary consequence of $\theta \rightarrow 0$.

Let us therefore write $\theta = e^{-a}$, $\lambda e^{-a\tau_0} = 1$, defining τ_0 , and assume that λ and therefore $a\tau_0$ are large. Then

$$\begin{aligned} \frac{h^3 G}{4\pi(2m)^{\frac{3}{2}} V} &= \int_0^{\tau_0} \{ \log(\lambda e^{-a\tau}) + \log(1 + e^{a\tau}/\lambda) \} \tau^{\frac{1}{2}} d\tau + \int_{\tau_0}^\infty \log(1 + \lambda e^{-a\tau}) \tau^{\frac{1}{2}} d\tau, \\ &= \frac{2}{3} (\log \lambda) \tau_0^{\frac{3}{2}} - \frac{2}{5} a \tau_0^{\frac{5}{2}} + \sum_{j=1}^\infty \frac{(-)^{j-1}}{j} \int_0^{\tau_0} e^{-ja\tau} (\tau_0 - \tau)^{\frac{1}{2}} d\tau \\ &\quad + \sum_{j=1}^\infty \frac{(-)^{j-1}}{j} \int_0^\infty e^{-ja\tau} (\tau_0 + \tau)^{\frac{1}{2}} d\tau. \end{aligned}$$

In transforming the integrals we have used the fact that $e^{a\tau_0} = \lambda$. Since $a\tau_0$, and *a fortiori* $ja\tau_0$, are large, we have approximately

$$\int_0^{\tau_0} e^{-ja\tau} (\tau_0 - \tau)^{\frac{1}{2}} d\tau \sim \frac{\tau_0^{\frac{3}{2}}}{ja}, \quad \int_0^\infty e^{-ja\tau} (\tau_0 + \tau)^{\frac{1}{2}} d\tau \sim \frac{\tau_0^{\frac{3}{2}}}{ja}.$$

Therefore finally (and approximately)

$$\frac{h^3 G}{4\pi(2m)^{\frac{3}{2}}V} = \frac{4}{15} \frac{(\log \lambda)^{\frac{5}{2}}}{(\log 1/\theta)^{\frac{3}{2}}} + \frac{\pi^2}{6} \frac{(\log \lambda)^{\frac{1}{2}}}{(\log 1/\theta)^{\frac{3}{2}}},$$

an equation whose real value can be expressed in the form

$$\frac{h^3 G}{4\pi(2m)^{\frac{3}{2}}V} = \frac{4}{15} \frac{(\log \lambda)^{\frac{5}{2}}}{(\log 1/\theta)^{\frac{3}{2}}} + O\left\{\frac{(\log \lambda)^{\frac{1}{2}}}{(\log 1/\theta)^{\frac{3}{2}}}\right\} \quad (9)$$

This equation can be differentiated. Therefore

$$\frac{h^3 N}{4\pi(2m)^{\frac{3}{2}}V} = \frac{2}{3} \frac{(\log \lambda)^{\frac{5}{2}}}{(\log 1/\theta)^{\frac{3}{2}}} + O\left\{\frac{1}{(\log \lambda)^{\frac{1}{2}} (\log 1/\theta)^{\frac{3}{2}}}\right\} \quad (10)$$

$$\frac{h^3 E_e}{4\pi(2m)^{\frac{3}{2}}V} = \frac{2}{5} \frac{(\log \lambda)^{\frac{5}{2}}}{(\log 1/\theta)^{\frac{3}{2}}} + O\left\{\frac{(\log \lambda)^{\frac{1}{2}}}{(\log 1/\theta)^{\frac{3}{2}}}\right\} \quad (11)$$

It follows from (10) that $\theta \rightarrow 0$ implies $\lambda \rightarrow \infty$, and therefore from (10) and (11) combined that as $\theta \rightarrow 0$

$$\frac{h^3 E_e}{4\pi(2m)^{\frac{3}{2}}V} \sim \frac{2/3}{5} \left(\frac{h^3 N}{4\pi(2m)^{\frac{3}{2}}V} \right)^{\frac{5}{2}} \quad (12)$$

The temperature can (and presumably would) fall to zero, still leaving, however, the free electrons with kinetic energy E_e given by (12). Similar relations hold for the nuclei. In the limit, however, $E_q = E_e(m/qm_q)$, and is therefore negligible compared with E_e .

It is wise to check up the behaviour of the entropy meanwhile. The contribution S_e by the electrons is given by

$$\begin{aligned} S_e &= k[G - N \log \lambda + E_e \log 1/\theta], \\ &= k \frac{4\pi(2m)^{\frac{3}{2}}V}{h^3} \left[\frac{(\log \lambda)^{\frac{5}{2}}}{(\log 1/\theta)^{\frac{3}{2}}} \left\{ \frac{4}{15} - \frac{2}{3} + \frac{2}{5} \right\} + O\left\{ \frac{(\log \lambda)^{\frac{1}{2}}}{(\log 1/\theta)^{\frac{3}{2}}} \right\} \right], \\ &\rightarrow 0. \end{aligned}$$

The contribution by the nuclei behaves in a similar way. This is correct.

We may note that the relation (12) can be confirmed by a direct calculation of the tight-packed phase-space required to house the N electrons. If the number of cells is the number of electrons, we find from (7)

$$\frac{N}{V} = \frac{4\pi(2m)^{\frac{3}{2}}}{h^3} \int_0^{\tau_0} \tau^{\frac{1}{2}} d\tau,$$

while at the same time

$$\frac{E_e}{V} = \frac{4\pi(2m)^{\frac{3}{2}}}{h^3} \int_0^{\tau_0} \tau^{\frac{3}{2}} d\tau,$$

which together give (12).

§ 3. *Numerical Calculations.*—Let us assume that the density of the stellar matter is 10^x gm./c.c., and that it is composed on the average of iron atoms, at

$$\frac{6.06 \times 10^{23}}{56} \text{ per gm.,}$$

or

$$\frac{6.06 \times 10^{23+x}}{56} \text{ per c.c.}$$

Then for the free electrons,

$$\frac{N}{V} = \frac{6.06 \times 26}{56} 10^{23+x} = 2.81 \times 10^{23+x},$$

$$\log_{10} \frac{E_e}{V} = -27 + 0.539 + \frac{5}{3} \log_{10} \frac{N}{V} = 12.621 + \frac{5}{3}x.$$

For a density 10^5 gm./c.c.

$$E_e/V = 9.0 \times 10^{20} \text{ ergs.}$$

Thus the average kinetic energy of an electron in this state is $9.0 \times 10^{20}/2.81 \times 10^{28}$ ergs. This is the same as the average kinetic energy of an *electron* in an expanded gas at a temperature T , where

$$\frac{3}{2}kT = \frac{9.0}{2.81} \times 10^{-8},$$

or

$$T = 1.56 \times 10^8 \text{ }^\circ\text{K.}$$

It is not, however, possible to rest quite content with this result, since the electrostatic energy terms are far from negligible, and in fact of the same order of magnitude. We observe first that the equation (12) is based entirely on considerations of phase-space. There must be enough cells of extension h^3 in six-dimensional phase-space to accommodate all the electrons. The phase-space depends only on velocities, or kinetic energy of the electrons, and is independent of the potential energy terms. The relation (12), therefore, should continue to give the necessary kinetic energy of the electrons, at least reasonably accurately, even when the effects of potential energy are allowed for. We have therefore merely to attempt to estimate the negative potential energy in order to compare the total energy of the condensed matter with its total energy in an expanded form.

The negative potential energy of a normal atom can be estimated roughly but easily if we recall that the average negative potential energy of an orbit in an inverse square field is twice the negative (total) energy of the orbit. The total negative energy of an iron atom is* 17,800+12,400+2700+600 volts for the K, L, M, and N electrons respectively, or 33,500 volts in all. The negative potential energy of one atom may therefore be taken to be

$$67,000 \text{ volts or } 1.065 \times 10^{-7} \text{ ergs.}$$

* Hartree, *Proc. Camb. Phil. Soc.*, **22**, 464 (1924).

The negative potential energy of the atoms equivalent to 1 c.c. of density 10^x is therefore

$$1.15 \times 10^{15+x} \text{ ergs.}$$

For the standard case of density 10^5 the negative energy of the normal atoms is, therefore, say 10^{20} ergs.

The negative potential energy in the condensed form is far more difficult to estimate with any pretence to accuracy. If we simply say that at a density 10^x everything will on the whole be about $(10^{x-1})^{\frac{1}{3}}$ closer than it could be packed before in normal atoms, we might argue that the negative potential energy would be increased about $10^{(x-1)/3}$ times, and so become

$$5.3 \times 10^{14+\frac{4}{3}x} \text{ ergs.,}$$

or in the standard case 2.5×10^{21} . This, however, must grossly over-estimate the increase. For half the negative energy comes from the two K electrons normally at a distance of 2×10^{-10} cm. from the nucleus. The average distance of uniformly distributed electrons at a density 10^5 is as much as 6×10^{-10} cm., and there is no reason for two electrons on the average to sink into about $2 \times 10^{-10}/10^{4/3}$, that is 10^{-11} cm. They might be expected to get hardly any closer, and the L-electrons not to experience the full shrinkage. If the contribution of the two electrons corresponding to the K electrons is unaltered, the potential energy increase, and therefore roughly the potential energy, is halved, and the incomplete shrinkage of the L group would halve the energy again. A negative potential energy of 5×10^{20} is all that can be expected.

An alternative estimate may be made as follows. The average space assigned to each nucleus is a sphere of radius r_0 , where

$$1\left(\frac{4}{3}\pi r_0^3\right) = \frac{2.81}{2.6} \times 10^{22+x},$$

$$r_0 = 1.30 \times 10^{-\frac{23+x}{3}}.$$

Let us estimate the electrostatic energy by distributing 26 electrons uniformly between 0 and r_0 for each nucleus, assume that the field of a single point charge $26e$ acts on each and ignore their own repulsions. Such an average distribution is not far wrong, for it is known that there is comparatively little crowding of bound electrons owing to the quantum restrictions, or of free electrons owing to the energy condition.* Thus the negative energy per atom is

$$(26e)^2 \overline{\left(\frac{1}{r}\right)}, \quad \overline{\left(\frac{1}{r}\right)} = \frac{\int_0^{r_0} r dr}{\int_0^{r_0} r^2 dr} = \frac{3}{2} \frac{1}{r_0}.$$

The negative potential energy per c.c. of condensed matter is therefore

$$8.9 \times 10^{13+\frac{4}{3}x},$$

or in the standard case 4.1×10^{20} .

* For the latter see Eddington, *loc. cit.*, § 183.

All this is very uncertain; the best we can do is to conclude that the negative potential energy of the condensed form is about

$$10^{14 + \frac{4x}{3}},$$

or in the standard case 4.6×10^{20} ergs, compared with 10^{20} ergs when expanded in normal atoms. As a result the kinetic energy is perhaps twice the negative potential energy for a density 10^5 , and the expanded form could become a perfect gas of normal atoms at about $1.56 \times 10^8 \times \frac{1}{2} \times 26$ say 2×10^9 °K.

Whether or no by our rough calculations we can show that the total energy of the condensed form is large and positive for a density of 10^5 , we can certainly conclude that this must be true for still greater densities,

for the kinetic energy per unit volume varies as $10^{\frac{5x}{3}}$, and on almost any view the negative potential energy must vary ultimately like $10^{\frac{4x}{3}}$. There seems no reason to doubt that this inequality, kinetic energy greater than negative potential energy, is true over wider conditions than we can show by these arguments. For in condensed forms at somewhat lower densities the tight packing is presumably achieved at the expense of shifting only the more lightly bound electrons, and our estimates of increase in the negative potential energy will be greatly in excess.

§ 4. *Cognate Speculations.*—If we return to the condensed assembly of mass-points,*electrostatic forces ignored, we may note that the pressure is given by the thermodynamic equation

$$p = T \frac{\partial G}{\partial V}.$$

This is strictly the pressure exerted by the electrons, and obeys the usual perfect gas equation

$$p = \frac{2}{3} \left(\frac{E_e}{V} \right);$$

the contribution by the nuclei is trivial by comparison. Thus in the condensed limit

$$p = \frac{4}{15} \left(\frac{3}{2} \right)^{\frac{2}{3}} \frac{h^2}{(4\pi)^{\frac{2}{3}} 2m} \left(\frac{N}{V} \right)^{\frac{2}{3}}.$$

If the volume is a sphere of radius R, then

$$p \propto \frac{1}{R^5}.$$

This pressure represents an interaction of the assembly with the outside world which is in a sense due to the quantum restraints to which the assembly is subject.

Now an atom, especially an atom of an inert gas, is really condensed matter of just the same general type. The assembly, whether atom or stellar material, contains just so many cells of phase-space available

for electrons and every one of them is full. It is tempting to speculate as to whether the apparent forces of repulsion, which prevent the too close approach of atoms, molecules, or ions with closed configurations of electrons, may not be due in just the same way purely to the quantum constraints. The number of cells of the phase-space must be unaltered in an encounter, and the apparent force of repulsion represents just the work which must be done to make up by an increase in the velocity (or kinetic energy) space for a loss in the extent of the "space"-space. These forces of repulsion between condensed atomic configurations of electrons seem empirically to vary like $1/R^{10}$, or thereabouts, for a large variety of atoms. It would be too much to expect the pressure of the stellar material to follow exactly the same law. But the general forms of the two types of system are sufficiently alike, and the resemblance of their laws of interaction close enough to lead one to believe that the origin of this important part of inter-atomic forces is to be sought in this direction, in the quasi-thermodynamic consequences of the existence of the quantum constraints embodied in Pauli's principle.

The Constants of the Star-Streams from the Photographic Proper Motions of 3029 Stars. By W. M. Smart, M.A., D.Sc.

(Communicated by the Director, Cambridge Observatory.)

Summary.

1. The data are the proper motions—based on an average interval of about 22 years—of 3029 stars in 31 regions (formed into 8 groups), from photographs taken with the Sheepshanks Equatorial of 19.3 feet focal length.

2. The frequency curve of the proper motions in each group has been analysed into two drifts. It is found that the stars are nearly equally divided between the two drifts: 1577 belong to Drift I. and 1452 to Drift II.

3. The combination of the 8 groups leads to the following positions of the Drift apices:—

Drift I.: R.A. $88^{\circ}4$; Dec. $-11^{\circ}8$.

Drift II.: R.A. $289^{\circ}0$; Dec. $-73^{\circ}3$.

4. The co-ordinates of the vertex of star-streaming are—

R.A. $91^{\circ}7$; Dec. $+19^{\circ}9$,

which is about $2\frac{1}{2}^{\circ}$ from the galactic plane.

5. The co-ordinates of the solar apex are—

R.A. $273^{\circ}2$; Dec. $+43^{\circ}6$.

§ 1. In four previous papers,* it has been shown that the pheno-

* *M.N.*, **84**, 3, 123, 481; **85**, 433.

menon of star-streaming is a feature not only of the bright stars but also of the fainter and more remote stars. The evidence is, in effect, contained in each of the 16 diagrams of the papers quoted. Fifteen additional photographic regions have now been measured—bringing the total up to 31—and altogether the proper motions of 3029 stars have been derived. Neighbouring regions have been grouped together, so that an adequate number of stars per group may be available for the analysis of the proper motions. In this way 8 groups have been formed, the details of which are given in Table I. The mean R.A. and declination, for 1900, of each of the groups are shown in columns 2 and 3. The last column gives the weights assigned to the groups.

TABLE I.

Group.	R.A.		Dec.		Number of Regions.	Number of Stars.	Galactic Latitude.	Weight.
	h	m	°	'				
I.	0	16.5	+50	0	3	545	-11	4
II.	1	47.3	+48	14	7	381	-10	3
III.	3	16.0	+41	9	4	306	-12	2½
IV.	5	55.9	+29	42	4	607	+5	3
V.	11	6.0	+29	24	3	126	+62	1
VI.	19	31.3	+26	33	3	524	+2	4
VII.	22	12.0	+57	57	3	284	0	3
VIII.	23	6.0	+25	47	4	256	-31	2

§ 2. It may be advantageous to give here a brief summary of the method adopted for deriving the photographic proper motions. The plates of the earlier epoch are mainly parallax plates taken by A. R. Hinks and H. N. Russell between 1900 and 1906. The plates, with which these parallax plates have been compared, were photographed through the glass so that, by placing the plates film to film, direct comparisons of the positions of a stellar image on each of the parallax and "reversed" plates are easily made in the measuring machine. The intervals, on which the proper motions depend, are on the average about 22 years. Group II., in Table I. above, consists entirely of Eros fields photographed by Mr. Hinks at the 1900-1 opposition, and the intervals between the comparison plates are about 25 years. The exposures given on the Eros plates are only 1 minute, but all the B.D. stars and stars down to magnitude 10 approximately are shown on the photographs.

The apparent displacements of each star are measured (*a*) parallel to the equator; (*b*) in declination. These measures are reduced by means of linear formulæ according to the method of Christie and Dyson,* 20 to 40 comparison stars being chosen according to the nature of the field under consideration. We then derive the proper motions of all the stars on the plate relatively to the group of comparison stars. The number of stars, in any region, with well-determined meridian proper motions is generally inadequate for the conversion of relative proper

* *M.N.*, 55, 61.

motions into absolute proper motions—only in 3 regions could this method be compared with that actually adopted. The method employed consists in comparing the parallactic motions of a group of stars—generally the B.D. stars—of known mean magnitude m , with the observed relative proper motions of the group. The mean parallaxes of stars of magnitude m —on which this procedure is based—have been taken from Kapteyn's results.* The adopted position of the solar apex is—

R.A. 270° ; Dec. $+34^\circ$.

§ 3. The numbers of stars are counted, for each region, whose absolute proper motions lie in each of the thirty-six position-angle sectors of 10° ; the regions of a group are combined, and the result is Table II. The curves which exhibit the distribution of proper motions in position-angle have been smoothed by taking the means of each 10° sector with its two adjacent sectors. The frequency curves of the eight groups are to be found below.

TABLE II.

Distribution of Proper Motions in Position-Angle.

Position Angle.	Group I.	Group II.	Group III.	Group IV.	Group V.	Group VI.	Group VII.	Group VIII.
10°	7	2	1	2	1	7	2	0
20°	3	1	1	5	0	7	2	2
30°	7	3	1	4	0	9	8	1
40°	7	5	2	4	1	9	10	6
50°	6	7	1	8	0	15	21	9
60°	14	5	7	8	0	24	17	14
70°	16	11	5	8	1	21	19	12
80°	27	17	6	9	1	16	26	18
90°	34	26	10	13	3	18	16	20
100°	50	21	14	13	3	19	13	16
110°	40	36	18	15	4	22	11	9
120°	40	38	22	18	4	16	11	12
130°	33	31	24	26	3	16	4	9
140°	28	21	29	29	4	17	7	14
150°	31	17	25	27	4	28	9	9
160°	20	15	19	45	4	29	7	8
170°	15	11	16	54	4	28	8	11
180°	15	15	15	51	4	29	12	14
190°	29	15	9	48	6	35	5	8
200°	23	14	11	44	7	35	12	9
210°	19	11	13	35	5	21	7	13
220°	14	8	9	35	5	20	7	5

* *Groningen Publications*, No. 29, table xxvi.

TABLE II.—*continued.*

Position Angle.	Group I.	Group II.	Group III.	Group IV.	Group V.	Group VI.	Group VII.	Group VIII.
230°	16	5	10	20	5	12	10	9
240°	12	7	3	20	6	7	8	5
250°	4	6	3	12	11	11	4	1
260°	5	5	6	10	6	5	3	2
270°	4	3	5	9	8	6	2	3
280°	3	3	5	8	10	2	1	0
290°	3	2	3	5	7	5	2	2
300°	3	3	6	4	2	6	3	1
310°	3	2	1	3	0	3	1	1
320°	3	3	0	1	3	4	2	3
330°	1	2	1	3	0	3	4	3
340°	3	1	3	4	2	3	2	3
350°	2	2	0	3	2	6	4	2
360°	5	7	2	4	0	10	4	1

§ 4. The magnitudes of the stars, with which this paper deals, are roughly between magnitudes 8 and 12, and it is evident that for a considerable percentage of the stars the proper motions will be extremely small; in fact, the larger proper motions, of 10" per century and over, just average 2 in number per region. It is, consequently, of importance to ascertain with what precision the proper motions have been derived. Before considering this question of the probable errors of the proper motions, I give in the following table the numbers of stars with total proper motions between various limits of magnitude—the proper motions are expressed in seconds of arc per century. Only 12 per cent. of the stars, according to the table, have proper motions exceeding 4" per century.

TABLE III.

The Magnitudes of the Proper Motions.

Total Proper Motion.	Group I.	Group II.	Group III.	Group IV.	Group V.	Group VI.	Group VII.	Group VIII.	Total.
0°-1°	87	87	68	117	13	141	90	43	646
1°-2°	175	131	105	225	23	201	96	85	1041
2°-3°	146	83	72	146	26	116	48	60	697
3°-4°	57	27	21	66	17	38	22	33	281
4°-5°	33	12	14	17	17	10	9	7	119
5°-6°	17	12	12	12	12	3	8	3	79
6°-7°	8	7	8	12	5	5	2	6	53
7°-8°	4	1	0	2	3	3	4	4	21
8°-9°	3	3	2	3	2	2	1	2	18
9°-10°	3	4	0	0	0	0	1	2	10
Over 10°	12	14	4	7	8	5	3	11	64

It is evident, then, that the statistical results of this paper would be open to serious criticism if the probable error of the proper motions turned out to be, say, of the order of 1" per century. A great deal, clearly, depends on the quality of the plates. The early parallax plates are generally of excellent quality, as demanded by the exacting requirements of the work for which they were employed. The best plate of a parallax series was always taken as a standard of excellence, to which the "reversed" plates were expected to attain. Of these latter plates, only the best were measured. Throughout the work, measures of pairs of plates were made independently by Mr. H. E. Green and the writer, and the means of the two sets of measures taken. Generally, comparison of 4 to 8 pairs of images of any one star were made.

The internal discordances of the measures, or rather of the deduced proper motions, furnish material for arriving at an estimate of the probable errors. In a previous paper* I have given the proper motions of 28 stars which are common to two overlapping regions, each region being reduced in the usual way, and the average probable errors of the mean proper motions are found to be of the order of 0".2 per century in each co-ordinate. This is, however, an exceptional instance of reaching an estimate of probable error, and, consequently, it is necessary to fall back upon the internal discordances of a series of measures for each individual star.

The probable errors of the stars in the 3 regions of Group I. are given below. The stars are divided into groups according to the measured diameters of the images and the means taken.

Region of λ Andromedæ.

Diameter	<15	15-17	17-19	19-21	21-27	> 27
R.A.	0".50	"44	"38	"42	"33	"30
Dec.	0.51	0.40	0.34	0.33	0.23	0.24

Region of η Cassiopeiæ.

Diameter	<12	12-14	14-16	16-18	18-20	> 20
R.A.	0".50	"42	"44	"40	"46	"35
Dec.	0.48	0.38	0.40	0.45	0.30	0.30

Region of Groombridge 34.

Diameter	<12	13, 14	15, 16	17, 18	19, 20	21-24	25-28	> 28
R.A.	0".38	"30	"35	"21	"30	"20	"27	"35
Dec.	0.35	0.34	0.26	0.22	0.24	0.14	0.25	0.32

The mean probable error in each co-ordinate for all the stars appears to be, approximately, 0".4 per century. The measures in declination are just a trifle better than the measures in R.A.

* *M.N.*, 85, 438.

For the Eros fields, the average probable error in each co-ordinate is somewhat smaller—approximately $0''.3$ per century, according to a rough calculation—due to the much longer interval, namely, 25 years.

The conclusion seems to be that the probable errors do not, on the average, exceed $0''.4$ per century. These are accidental errors. In addition, there are the systematic errors arising from the “parallactic correction” applied to the relative proper motions—it is unlikely that such systematic errors exceed $0''.4$ per century.

§ 5. The analysis of the frequency curves into two drifts follows the method* of Professor Eddington. If, in drift motion, $\rho d\theta$ is the number of stars moving between position-angles θ and $\theta + d\theta$, then it is shown that

$$\rho = \frac{N}{\pi} e^{-h^2 V^2} \left\{ \frac{1}{2} + \tau e^{\tau^2} \int_{-\tau}^{\infty} e^{-x^2} dx \right\},$$

where N is the total number of stars in the drift, V is the linear velocity of the drift (in terms of the theoretical unit $1/h$) at right angles to the line of sight in the position-angle θ_0 , and $\tau = hV \cos(\theta - \theta_0)$. Writing

$$f(\tau) = \frac{2}{\sqrt{\pi}} \left\{ \frac{1}{2} + \tau e^{\tau^2} \int_{-\tau}^{\infty} e^{-x^2} dx \right\}$$

we have

$$\rho = \frac{N}{2\sqrt{\pi}} e^{-h^2 V^2} f(\tau),$$

The values of the function $f(\tau)$ have been tabulated by Professor Eddington in the paper just referred to.

If r denotes the number of stars moving in the sector $\theta - 5^\circ$ to $\theta + 5^\circ$, and if ρ is taken to be the mean value throughout the sector, then

$$r = 0.04923 N \phi(hV, \theta - \theta_0).$$

where

$$\phi = e^{-h^2 V^2} f(\tau).$$

Table IV. gives the values of the function $\phi(hV, \theta - \theta_0)$ for successive values of $\theta - \theta_0$ at 5° intervals, and for values of hV up to 1.8 at intervals of 0.1. When the frequency curve is a composite of two drifts (as is assumed here), we have that R , the total number of stars of the field moving in the sector $\theta - 5^\circ$ to $\theta + 5^\circ$, is given by $R = r_1 + r_2$, where

$$r_1 = 0.04923 N_1 \phi(hV_1, \theta - \theta_1)$$

$$r_2 = 0.04923 N_2 \phi(hV_2, \theta - \theta_2),$$

in which N_1 , hV_1 , θ_1 , and N_2 , hV_2 , θ_2 , are the constants of the two drifts respectively, and $N_1 + N_2$ is the total number of stars in the region considered. As the observed frequency curve has been smoothed by taking the means of three adjacent sectors of 10° , the calculated frequency curve has been treated in the same way—the main effect results in making the theoretical maxima a trifle less pronounced.

* *M.N.*, 67, 34.

TABLE IV.

Values of $\varphi(hV, \theta - \theta_0)$.

$\overbrace{hV.}^{\theta - \theta_0}$		0.1.	0.2.	0.3.	0.4.	0.5.	0.6.	0.7.	0.8.	0.9.
0	360	0.67	0.79	0.91	1.05	1.20	1.36	1.52	1.69	1.87
5	355	0.67	0.78	0.91	1.05	1.19	1.35	1.50	1.67	1.84
10	350	0.67	0.78	0.90	1.04	1.18	1.34	1.48	1.64	1.80
15	345	0.67	0.77	0.90	1.02	1.16	1.30	1.44	1.58	1.71
20	340	0.66	0.76	0.88	1.00	1.13	1.26	1.37	1.50	1.61
25	335	0.65	0.75	0.87	0.97	1.08	1.20	1.30	1.40	1.48
30	330	0.65	0.74	0.85	0.94	1.03	1.13	1.21	1.30	1.36
35	325	0.64	0.73	0.83	0.90	0.98	1.06	1.12	1.18	1.22
40	320	0.64	0.72	0.80	0.86	0.93	0.98	1.02	1.06	1.07
45	315	0.63	0.70	0.77	0.82	0.87	0.91	0.94	0.95	0.95
50	310	0.63	0.69	0.74	0.78	0.81	0.83	0.85	0.84	0.82
55	305	0.62	0.67	0.71	0.74	0.76	0.77	0.76	0.75	0.71
60	300	0.61	0.65	0.68	0.70	0.70	0.70	0.68	0.65	0.62
65	295	0.60	0.63	0.65	0.66	0.65	0.64	0.61	0.57	0.53
70	290	0.59	0.62	0.62	0.62	0.60	0.58	0.54	0.51	0.45
75	285	0.58	0.60	0.59	0.58	0.55	0.52	0.48	0.44	0.39
80	280	0.57	0.58	0.56	0.54	0.51	0.48	0.43	0.38	0.33
85	275	0.57	0.56	0.54	0.51	0.48	0.43	0.39	0.34	0.29
90	270	0.56	0.54	0.51	0.48	0.44	0.39	0.35	0.30	0.25
95	265	0.55	0.52	0.49	0.45	0.41	0.36	0.31	0.27	0.22
100	260	0.54	0.51	0.47	0.43	0.38	0.33	0.28	0.23	0.19
105	255	0.53	0.50	0.45	0.40	0.35	0.30	0.25	0.21	0.17
110	250	0.52	0.48	0.43	0.38	0.33	0.28	0.23	0.19	0.15
115	245	0.52	0.47	0.42	0.36	0.31	0.26	0.21	0.17	0.14
120	240	0.51	0.46	0.40	0.34	0.29	0.24	0.19	0.16	0.12
125	235	0.50	0.44	0.38	0.33	0.27	0.22	0.18	0.14	0.11
130	230	0.50	0.43	0.37	0.31	0.26	0.21	0.17	0.13	0.10
135	225	0.49	0.43	0.36	0.30	0.25	0.20	0.16	0.13	0.09
140	220	0.49	0.42	0.35	0.29	0.24	0.19	0.15	0.12	0.09
145	215	0.48	0.41	0.34	0.28	0.23	0.18	0.14	0.11	0.08
150	210	0.48	0.41	0.33	0.27	0.22	0.17	0.13	0.10	0.08
155	205	0.48	0.40	0.33	0.27	0.21	0.17	0.13	0.10	0.08
160	200	0.47	0.39	0.32	0.26	0.21	0.16	0.12	0.10	0.07
165	195	0.47	0.39	0.32	0.26	0.20	0.16	0.12	0.09	0.07
170	190	0.47	0.39	0.32	0.25	0.20	0.16	0.12	0.09	0.07
175	185	0.47	0.39	0.31	0.25	0.20	0.16	0.12	0.09	0.07
180	180	0.47	0.39	0.31	0.25	0.20	0.16	0.12	0.09	0.07

TABLE IV.—*continued.**Values of $\varphi(hV, \theta - \theta_0)$.*

$\begin{array}{c} hV. \\ \theta - \theta_0. \end{array}$		I.0.	I.1.	I.2.	I.3.	I.4.	I.5.	I.6.	I.7.	I.8.
0	360	2.05	2.24	2.43	2.62	2.81	3.01	3.21	3.40	3.60
5	355	2.02	2.21	2.38	2.57	2.76	2.94	3.15	3.32	3.51
10	350	1.97	2.14	2.28	2.44	2.62	2.77	2.94	3.10	3.27
15	345	1.86	2.05	2.12	2.25	2.38	2.50	2.63	2.72	2.85
20	340	1.73	1.87	1.93	2.02	2.10	2.17	2.25	2.28	2.32
25	335	1.57	1.65	1.70	1.76	1.80	1.82	1.87	1.86	1.82
30	330	1.41	1.46	1.48	1.50	1.50	1.44	1.46	1.42	1.40
35	325	1.24	1.25	1.27	1.25	1.22	1.17	1.07	1.08	1.05
40	320	1.08	1.07	1.04	1.03	0.98	0.93	0.86	0.80	0.74
45	315	0.94	0.90	0.87	0.82	0.76	0.70	0.64	0.57	0.51
50	310	0.80	0.76	0.70	0.65	0.59	0.53	0.47	0.41	0.35
55	305	0.68	0.63	0.58	0.52	0.46	0.40	0.34	0.28	0.24
60	300	0.57	0.51	0.46	0.40	0.35	0.30	0.25	0.20	0.16
65	295	0.48	0.43	0.37	0.31	0.27	0.22	0.18	0.14	0.11
70	290	0.40	0.35	0.30	0.25	0.21	0.17	0.13	0.10	0.08
75	285	0.34	0.29	0.24	0.20	0.16	0.13	0.10	0.07	0.06
80	280	0.29	0.24	0.20	0.16	0.13	0.10	0.08	0.05	0.04
85	275	0.24	0.20	0.16	0.13	0.10	0.08	0.06	0.04	0.03
90	270	0.21	0.17	0.13	0.10	0.08	0.06	0.04	0.03	0.02
95	265	0.18	0.14	0.11	0.08	0.06	0.05	0.04	0.02	0.02
100	260	0.15	0.12	0.10	0.07	0.05	0.05	0.04	0.02	0.01
105	255	0.14	0.11	0.08	0.06	0.05	0.04	0.03	0.02	0.01
110	250	0.12	0.09	0.07	0.05	0.04	0.04	0.03	0.02	0.01
115	245	0.11	0.08	0.06	0.04	0.04	0.03	0.03	0.02	0.01
120	240	0.09	0.07	0.05	0.04	0.03	0.03	0.03	0.01	0.01
125	235	0.09	0.06	0.05	0.04	0.03	0.02	0.02	0.01	0.01
130	230	0.08	0.06	0.04	0.03	0.02	0.02	0.02	0.01	0.01
135	225	0.07	0.05	0.04	0.03	0.02	0.02	0.02	0.01	0.01
140	220	0.07	0.05	0.04	0.03	0.02	0.02	0.02	0.01	0.01
145	215	0.06	0.05	0.04	0.03	0.02	0.02	0.02	0.01	0.01
150	210	0.06	0.05	0.03	0.03	0.02	0.02	0.02	0.01	0.01
155	205	0.06	0.04	0.03	0.03	0.02	0.02	0.02	0.01	0.01
160	200	0.05	0.04	0.03	0.03	0.02	0.02	0.01	0.01	0.01
165	195	0.05	0.04	0.03	0.03	0.02	0.02	0.01	0.01	0.01
170	190	0.05	0.04	0.03	0.03	0.02	0.02	0.01	0.01	0.01
175	185	0.05	0.04	0.03	0.03	0.02	0.02	0.01	0.01	0.01
180	180	0.05	0.04	0.03	0.03	0.02	0.02	0.01	0.01	0.01

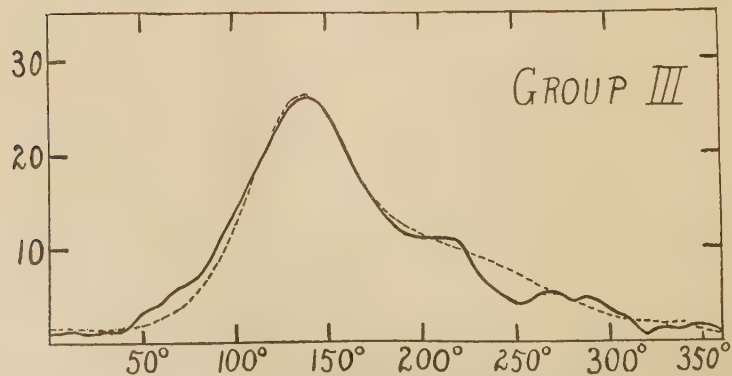
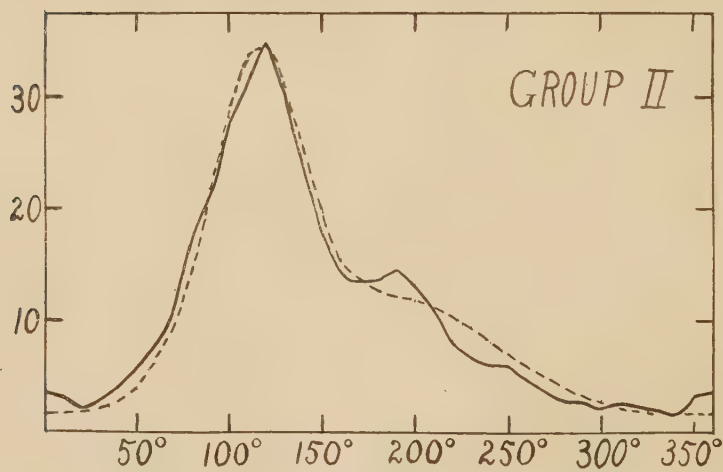
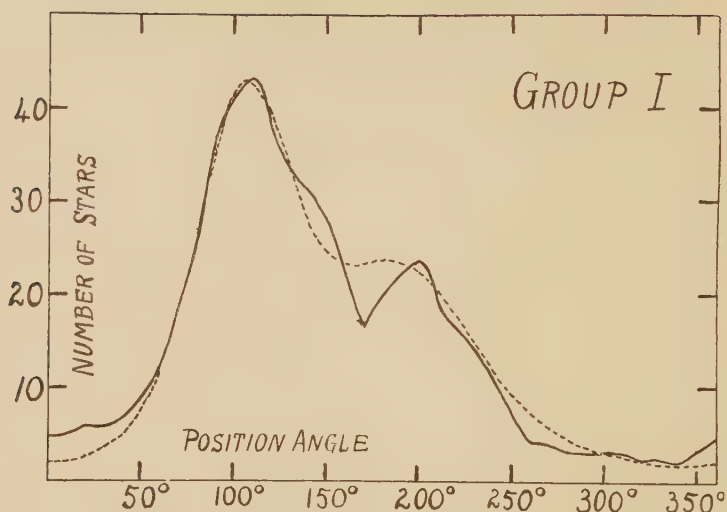


FIG. 1.—(Groups I, II, III). Observed distribution of proper motions (full line): theoretical distribution (dotted line).

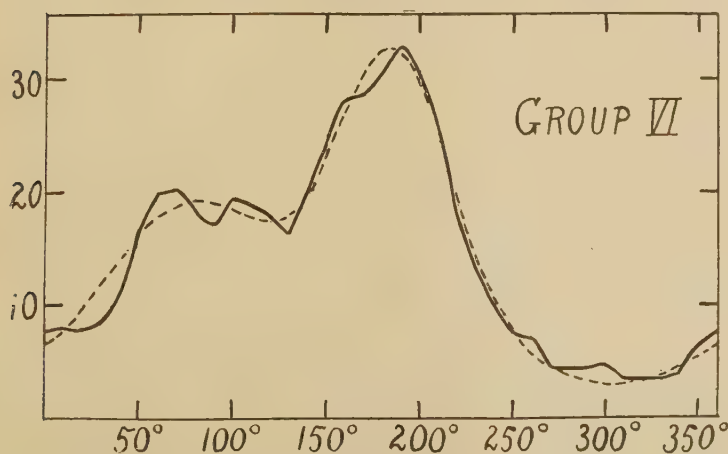
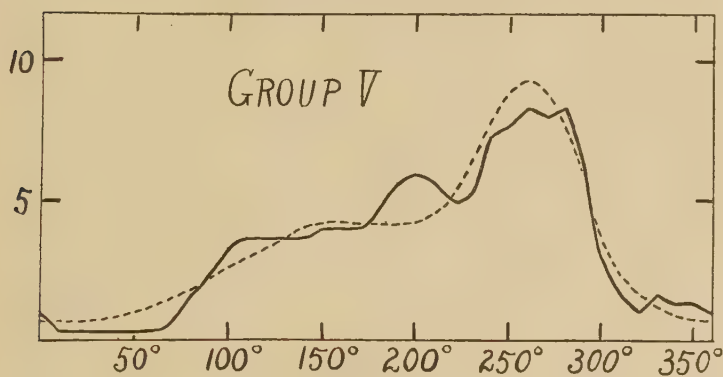
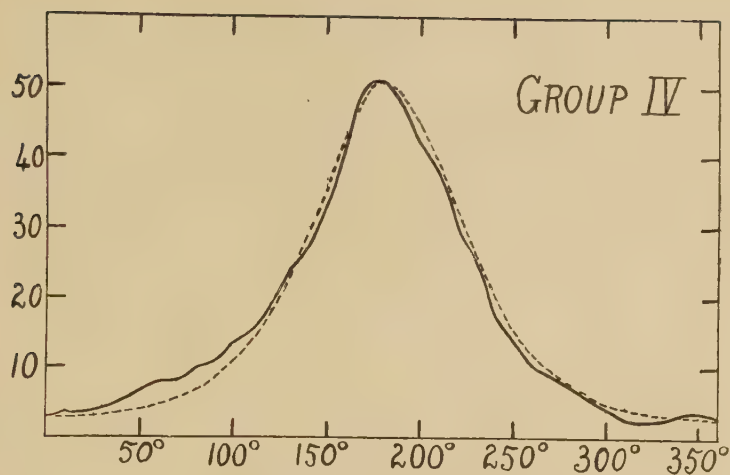


FIG. 2.—(Groups IV, V, VI). Observed distribution of proper motions (full line): theoretical distribution (dotted line).

In most of the groups Drift I. is prominent, and from the form of the curve and the direction in which it is pointing, it is usually easy to obtain, almost at the first trial, fairly accurate values of N_1 , θ_1 , and hV_1 . On the other hand, in many of the groups, Drift II.—owing to

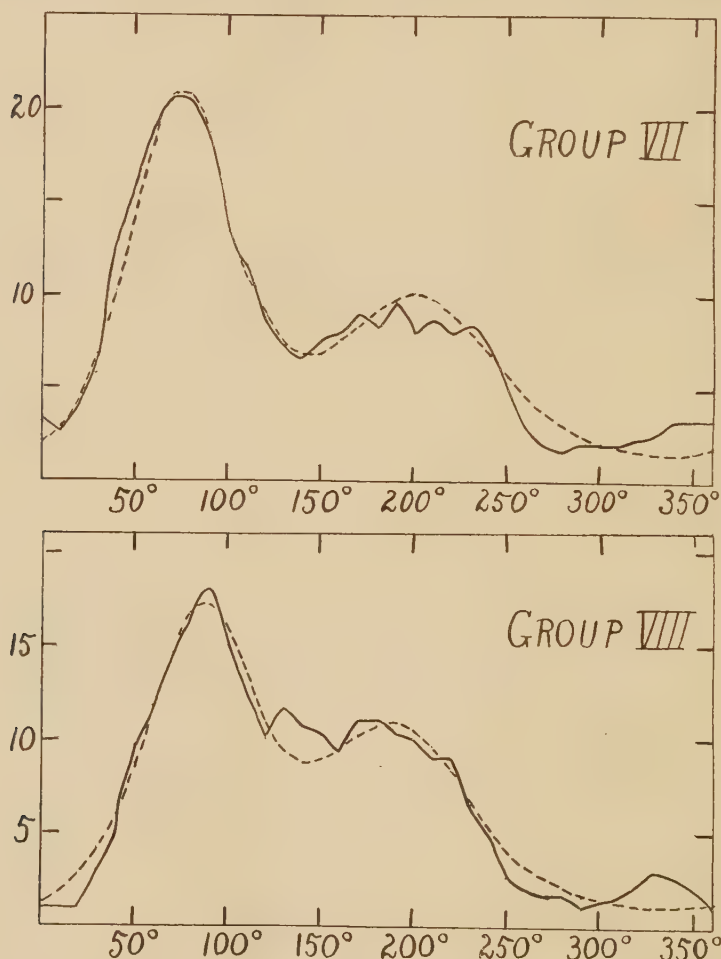


FIG. 3.—(Groups VII, VIII). Observed distribution of proper motions (full line): theoretical distribution (dotted line).

the much smaller values of hV_2 —gives a much flatter curve and, consequently, it is much more difficult to ascertain reliable values of θ_2 . The converse holds for Group VI, in which Drift II. is predominant owing to the close proximity of the region to the apex of Drift I. The process of deriving a theoretical curve to represent the observed curve is essentially one of “trial and error,” and occasionally as many as

six complete solutions for a particular group have been made. The constants of the drifts finally adopted are those which appear to give the best fit between the observed and the calculated frequency curves. The diagrams which follow show the observed distribution of proper motions (full line) and the theoretical distribution (dotted line) found, by "trial and error," to give the closest representation to the observed curve. The abscissæ are position-angles, and the ordinates are the numbers of stars with proper motions in the various position-angles.

§ 6. The co-ordinates of the apices of Drift I. and Drift II. have been calculated in two ways. In this section, the results are based entirely on the values of θ_1 and θ_2 obtained from the analysis of the frequency curves. For each group we have a great circle—through the centre of the group—giving the direction in which Drift I. is pointing, and the method consists simply in finding analytically the position on the sphere—the apex—which best represents the point of convergency of the eight great circles. The procedure for determining the apex of Drift II. is similar. The method is analogous to Bessel's method for deriving the co-ordinates of the solar apex from individual proper motions.

If S denotes the mean position of a group on the celestial sphere, P the north pole and ST a great circle in position-angle θ with respect to SP, then, on the hypothesis that the drift directions for all the eight groups converge to a point—the apex of the drift—the locus of the poles Q of the eight great circles ST, associated, for example, with Drift I., will be a great circle on the sphere. Let a, d denote the R.A. and declination of a pole Q (taken in the northern hemisphere), and α, δ the corresponding co-ordinates for the centre S of the group. Then the following equations give the co-ordinates a, d in terms of α, δ , and θ :—

$$\begin{aligned}\sin d &= \cos \delta \sin \theta, \\ \cot(a-\alpha) &= \sin \delta \tan \theta,\end{aligned}$$

with

$$\sin(a-\alpha) = -\cos \theta \sec d$$

as a check equation.

The poles Q lie on a plane

$$lx + my = z.$$

Suppose λ, ϕ are the north polar distances of the points in which this plane cuts the meridians $\alpha = 0$ and $\alpha = 90^\circ$ respectively. Then it follows that

$$l = \cot \lambda \quad \text{and} \quad m = \cot \phi.$$

The equation of the great circle—the locus of the poles Q—is then

$$\cot \lambda \cos \alpha \cos \delta + \cot \phi \sin \alpha \cos \delta = \sin \delta.$$

One of the poles of this great circle is the apex of the drift—let its co-ordinates be A, D. Then it is easily found that

$$\begin{aligned}\sin A &= -\cot \phi \tan D, \\ \cos A &= -\cot \lambda \tan D.\end{aligned}$$

The procedure is to calculate the co-ordinates of the poles Q, then to find their equatorial rectangular co-ordinates x , y , and z , and then to solve for l and m by least squares from the eight equations of the form $lx + my = z$. Thus the great circle, whose pole is the apex, is obtained, and, from it, the co-ordinates of the apex.

In Table I. are found the weights assigned to each group—based partly on the number of stars and partly on the ease or difficulty in separating the drifts.

The solutions are—

Drift I.	$l_1 = -0.172$	± 0.159
	$m_1 = +2.809$	± 0.702
	$A_1 = 93^\circ.5$	$\pm 3^\circ.3$
	$D_1 = -19^\circ.6$	$\pm 4^\circ.5$
	$r_1 = \pm 0.411$	

Drift II.	$l_2 = +0.056$	± 0.045
	$m_2 = -0.251$	± 0.037
	$A_2 = 282^\circ.6$	$\pm 9^\circ.9$
	$D_2 = 75^\circ.6$	$\pm 2^\circ.0$
	$r_2 = \pm 0.129$	

where r_1 and r_2 are the probable errors of an observational equation of unit weight. The apparently large probable error found for A_2 is to be explained partly by the high declination of the apex of Drift II.

The disadvantage of the method is that it makes use of only part of the observed drift constants, namely, only the position-angles θ_1 and θ_2 . In the following section a solution is made which takes into account the observed values of θ_1 , θ_2 , and hV_1 , hV_2 .

§ 7. The second method employed for determining the drift constants is analogous to Airy's method for finding the solar motion from proper motions. If X , Y , Z are the linear velocities in space of a drift—referred to equatorial rectangular axes—and if α , δ are the co-ordinates of the centre of a group, then

$$\begin{aligned} -X \sin \alpha + Y \cos \alpha &= hV \sin \theta \\ X \cos \alpha \sin \delta + Y \sin \alpha \sin \delta - Z \cos \delta &= -hV \cos \theta. \end{aligned}$$

There are thus eight pairs of equations of the above form for each drift. From these equations, X , Y , and Z are found by a least-squares solution, the weights assigned to the groups being those given in Table I.

The results are—

	Drift I.		Drift II.	
X	+0.041	± 0.033	+0.091	± 0.035
Y	+1.465	± 0.025	-0.265	± 0.031
Z	-0.307	± 0.036	-0.934	± 0.038

which lead to the following co-ordinates of the apices and to the space

velocities of the drifts (relative to the sun) in terms of the theoretical unit $1/h$:—

	Drift I.		Drift II.	
R.A. of Apex	88°·4	$\pm 1^{\circ} \cdot 5$	289°·0	$\pm 7^{\circ} \cdot 0$
Dec. „	-11°·8	$\pm 1^{\circ} \cdot 4$	-73°·3	$\pm 1^{\circ} \cdot 9$
hV	1·497	$\pm 0 \cdot 025$	0·975	$\pm 0 \cdot 037$

The probable error for the R.A. of the apex of Drift II. is again large, for the same reason as in the preceding section, namely, the high value of the declination of the apex.

For comparison, I add the results * of Kapteyn, Eddington, and Dyson—these are for the brighter stars of the sky—and G. H. ten Bruggencate † for stars down to magnitude 12.

Bruggencate's results are based on the photographic proper motions derived at the Kapteyn Astronomical Laboratory from 33 regions, 9 of which are in the northern hemisphere, and the remainder are included in a narrow zone of mean declination -45° . The frequency curves were analysed by Dyson's method, ‡ which gives only the directions of the drifts but does not give their speeds.

The last line gives the results of this paper.

	Drift I. Apex.		Drift II. Apex.	
	α .	δ .	α .	δ .
Kapteyn	85°	-11°	260°	-48°
Eddington	91	-15	288	-64
Dyson	93	-7	246	-64
Bruggencate	88	-10	300	-50
Smart	88	-12	289	-73

The most noticeable feature of my results is the much higher declination of the apex of Drift II.

I add two tables, the first of which (Table V.) shows the comparison between the observed and calculated values of the position-angles θ_1 and θ_2 of the drifts for the eight groups. I have calculated also the shortest angular distances D_1 and D_2 from the respective apices to the great circles defined by the observed values of θ_1 and θ_2 —this is the best criterion for exhibiting the convergency of the great circles given by θ_1 and θ_2 . The values of D_1 and D_2 are in columns 5 and 9 of Table V. Group VI. is a region about 27° from the ant-apex of Drift I.; Table V. shows that although $\theta_1(\text{cal.}) - \theta_1(\text{obs.})$ is 15° , yet the great circle given by $\theta_1(\text{obs.})$ passes within 7° of the apex of Drift I. Group VIII. shows a rather large discordance in D_1 .

The second table (Table VI.) shows the comparison between the observed and calculated velocities of the drifts. Let λ denote the angular distance of the centre of a group from the apex of a drift, then

$$hV (\text{calculated}) = hV \sin \lambda,$$

* See also Eddington's *Stellar Movements*, p. 125.

† *Bull. Astron. Inst. of the Netherlands*, No. 85 (1925 Sept. 30).

‡ *Proc. Roy. Soc. Edin.*, 28, 231.

the values of hV on the right-hand side being 1.497 and 0.975 for Drift I. and Drift II. respectively. The positions of the apices used in the calculations are those found in this section.

TABLE V.

The Calculated and Observed Values of the Position-Angles of the Drifts.

Group.	Drift I.				Drift II.			
	Position-Angle θ_1 .			D_1 .	Position-Angle θ_2 .			D_2 .
	Cald.	Obsd.	Cald.-Obsd.		Cald.	Obsd.	Cald.-Obsd.	
I.	101	105	- 4	0	202	190	+12	0
II.	119	115	+ 4	4	205	200	+ 5	3
III.	136	135	+ 1	1	202	215	-13	9
IV.	181	180	+ 1	0	188	185	+ 3	2
V.	254	260	- 6	6	162	155	+ 7	6
VI.	60	75	-15	7	181	185	- 4	4
VII.	74	75	- 1	1	196	200	- 4	3
VIII.	96	85	+11	10	195	190	+ 5	4

TABLE VI.

The Calculated and Observed Values of the Projected Drift Velocities.

Group.	Drift I.			Drift II.		
	$hV_1 \sin \lambda_1$.		λ_1 .	$hV_2 \sin \lambda_2$.		λ_2 .
	Calculated.	Observed.		Calculated.	Observed.	
I.	96.5	1.49	1.50	133.3	0.71	0.80
II.	80.9	1.48	1.55	137.8	0.64	0.60
III.	64.2	1.35	1.40	137.6	0.64	0.65
IV.	41.5	1.00	1.00	135.2	0.69	0.60
V.	85.7	1.49	1.40	127.2	0.77	0.60
VI.	152.7	0.68	0.70	99.9	0.96	1.10
VII.	113.3	1.37	1.40	134.6	0.70	0.70
VIII.	105.7	1.44	1.30	106.1	0.93	0.80

§ 8. From the data of the preceding section, the co-ordinates of the vertex of star-streaming are at once derived. Denote by ξ , η , ζ the components of the velocity of Drift I. relative to Drift II. Then

$$\begin{aligned}\xi &\equiv X_1 - X_2 = -0.050 & \pm 0.048 \\ \eta &\equiv Y_1 - Y_2 = +1.729 & \pm 0.039 \\ \zeta &\equiv Z_1 - Z_2 = +0.627 & \pm 0.053\end{aligned}$$

From these values are deduced—

$$\begin{array}{lll}\text{R.A. of vertex} & 91^\circ.7 & \pm 1^\circ.6 \\ \text{Dec. ,, ,,} & +19^\circ.9 & \pm 1^\circ.6\end{array}$$

Expressed in galactic co-ordinates, this position becomes—

Longitude of vertex	158°·5
Latitude „ „	+2°·4

Thus the axis of star-streaming lies practically in the galactic plane.

The velocity of one stream relatively to the other is 1·840 in terms of the theoretical unit.

I add, for comparison, the following results for the position of the vertex :—

	<i>Vertex.</i>	R.A.	Dec.
Kapteyn		91°	+13°
Eddington (Boss stars)		94	+12
Dyson (large proper motions)		88	+21
Smart		92	+20

§ 9. The apex of the solar motion referred to the 3029 stars of the two streams can now be derived. The additional data necessary is the proportion in which the stars are divided between the two streams. The numbers are as follows :—

Group	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Total.
Drift I.	273	216	176	327	63	244	150	128	1577
„ II.	272	165	130	280	63	280	134	128	1452

It should be noticed, however, that, for the purpose of deriving the parallactic motions of the B.D. stars of the various regions, the apex of the solar motion was assumed to be 270°, +34°. Consequently, the position of the solar apex found from the photographic proper motions is not quite an independent determination, for approximately 20 per cent. of all the stars dealt with in this paper are B.D. stars. However, the effect should not be considerable. The results are —

R.A. of solar apex	273°·2
Dec. „ „	+43°·6

The declination is some 10° or 14° higher than that generally found from discussions of the proper motions of the bright stars.

The speed of the solar motion is found to be 0·881 in terms of the theoretical unit 1/*h*. The value found by Professor Eddington* from the Boss proper motions is 0·908.

§ 10. A search has been made amongst the larger proper motions to discover if any of the stars might possibly belong to Oort's category of "high velocity stars." Oort found† that certain stars of high velocity (above a well-determined limit of velocity) are moving towards one hemisphere of the sky (the effect of the solar motion being removed). The apex is 8^h 20^m; -44°. A criterion based on proper motion alone is insufficient to discriminate finally between the ordinary stream-stars and these exceptional high velocity stars, but if there were any

* *M.N.*, 71, 35.

† *B.A.N.*, No. 23; *Groningen Publications*, No. 40.

faint stars of the latter kind, it might be expected that they would be revealed by reason of a large proper motion in a direction well away from the drift directions: at best, they could only be suspected to belong to this category—determination of parallax or radial velocity or both would be necessary to complete the criteria adopted by Oort. The search has proved entirely negative—that is to say, no star has been suspected not to belong to either of the streams. This conclusion is hardly a definite one owing to the unsufficiency of the only criterion that can be applied from the available data.

Observatory, Cambridge :
1926 December 1.

On the Analysis of the Cambridge Proper Motions.

By A. S. Eddington, F.R.S.

1. In 1908* I gave an analytical method of determining the constants of the star-streams from proper-motion data, which might be used as an alternative to my previous method of trial and error. The Cambridge proper motions, investigated by Dr. Smart in another paper in this number, seem suitable for a more favourable and extended trial of it than I have hitherto given; I have therefore analysed them by the second method and compared the results with Dr. Smart's. The interest lies partly in the comparison of the efficiency and systematic differences of the two methods, and partly in the check on the element of judgment which must be exercised in applying the older method. I take the opportunity of adding some general comments on the conclusions to be drawn from this series of proper motions.

In the method as originally given, the ratio N_1/N_2 of the numbers of stars in the two streams was treated as an unknown to be found in the analysis. In this respect the method has proved unsuccessful, and there is no doubt that it is ill-adapted for investigating this quantity. I here assume $N_1 = N_2$, thereby putting the investigation on the same footing as an analysis by Schwarzschild's ellipsoidal theory, which likewise ignores fore-and-aft asymmetry (if any). It will be seen from Smart's results that he finds a nearly equal distribution between the two streams.

2. The modified method for $N_1 = N_2$ may be summarised as follows: Regarding the number of stars moving in any direction θ as a function of θ , we calculate the first two harmonics in the Fourier Series and use the equations connecting these with the directions and speeds of the drifts. Thus we calculate from the observed statistics the two complex numbers

$$\begin{aligned} L &= \text{mean value of } e^{i\theta}, \\ M &= \text{mean value of } e^{2i\theta}. \end{aligned}$$

* *Monthly Notices*, 68, 588.

For a single drift of velocity hV_1 in direction θ_1 , we have

$$\begin{aligned} L &= C_1 e^{i\theta_1} = \zeta_1 P_1, \\ M &= D_1 e^{2i\theta_1} = \zeta_1 P_1^2, \end{aligned}$$

where C , D , ζ , $|P|$ are tabulated functions of hV (*loc. cit.*, p. 592) and

$$\arg P_1 = \theta_1.$$

Then for two drifts with $N_1 = N_2$, we shall have

$$\begin{aligned} {}^2L &= \zeta_1 P_1 + \zeta_2 P_2, \\ {}^2M &= \zeta_1 P_1^2 + \zeta_2 P_2^2. \end{aligned}$$

Let

$$\begin{aligned} k &= \frac{1}{2}(\zeta_1 + \zeta_2), \\ \alpha &= \sqrt{(\zeta_2/\zeta_1)}. \end{aligned}$$

Then we find

$$kP_1 = L + \alpha\sqrt{(Mk - L^2)},$$

$$kP_2 = L - \frac{1}{\alpha}\sqrt{(Mk - L^2)}.$$

The values of k and α change rather slowly with hV_1 and hV_2 , so that these equations are easily solved by successive approximation. We usually start with $k = 1.4$, $\alpha = 1$; we find P_1 and P_2 from the above equations, and hence by the table ζ_1 and ζ_2 ; and thence derive the values of k and α to be used in the next approximation.

3. The application of this method to the eight regions gives the following results, Smart's values by trial and error being added for comparison:—

TABLE I.

Group.	Analytical Method.				Trial and Error.			
	hV_1 .	θ_1 .	hV_2 .	θ_2 .	hV_1 .	θ_1 .	hV_2 .	θ_2 .
1	1.22	103°	0.67	181°	1.50	105°	0.80	190°
2	1.32	112	0.50	174	1.55	115	0.60	200
3	1.44	130	0.56	196	1.40	135	0.65	215
4	1.18	191	0.53	152	1.00	180	0.60	185
5	1.34	260	0.74	158	1.40	260	0.60	155
6	0.61	82	1.07	186	0.70	75	1.10	185
7	1.37	70	0.53	189	1.40	75	0.70	200
8	1.12	86	0.79	183	1.30	85	0.80	190

Using the same weights as in Dr. Smart's paper, a solution by least squares for the components of velocity of the two star-streams gives

$$\begin{aligned} X_1 &= 0.00 \pm 0.057, & X_2 &= -0.06 \pm 0.049, \\ Y_1 &= +1.33 \pm 0.050, & Y_2 &= -0.02 \pm 0.043, \\ Z_1 &= -0.33 \pm 0.062, & Z_2 &= -0.90 \pm 0.053. \end{aligned}$$

And the polar co-ordinates are found to be—

TABLE II.

	Analytical Method.			Trial and Error.		
	R.A.	Dec.	hV .	R.A.	Dec.	hV .
Stream I. apex	89°9	-13°8	1·373	88°4	-11°8	1·497
Stream II. apex	203	-86°0	0·906	289	-73°3	0·975
Vertex	87·4	+23°0	1·475	91·7	+19°9	1·840
Solar apex	272·4	+43°3	0·898	273·2	+43°6	0·881

Confining attention to the vertex and solar apex (since the stream apices merely give the same information in another form), we notice that there is complete agreement of the two methods as to the solar apex; the positions of the vertex are reasonably accordant; but hV for the vertex, *i.e.* the relative velocity of the two streams, is 20 per cent. lower according to the present method. The same tendency to give a smaller value of the stream velocity has appeared in former trials of the method.

I think there is no doubt that this difference is systematic—that the automatic “judgment” of the analytical method tends to assign less separation to the two streams than the conscious judgment of the curve-fitter. It does not necessarily follow that the judgment of the latter is inferior. It must be remembered that the stream velocity V is measured in terms of a unit $1/h$ expressing the mean peculiar velocity of the stars. It is well known that the peculiar velocity is different for different types, so that in a general mixture of stars there will be a systematic kind of deviation from the simple drift curve; in fact, the peak of the curve will be too sharp compared with the distribution at large angles from the peak. I think that this effect can be detected in Dr. Smart’s curves, and it accounts for the systematic difference of hV in the two methods which make different kinds of compromise for the irregularity. A similar effect might also be produced by accidental errors of the proper motions. The analytical method does not profess to follow a least-squares criterion of fit, and I believe that the compromise effected by human judgment is the fairer one.

As regards the probable accuracy of the results the method of trial and error has a distinct advantage. The probable errors per unit weight in the determinations of X , Y , Z are as follows:—

Stream I.	Smart ± 118 ,	Eddington ± 215 .
Stream II.	Smart ± 123 ,	Eddington ± 174 .

The usual method of trial and error thus justifies the additional labour involved; the automatic method, which dispenses with human judgment, does not reach the same delicacy.

This result has a side-interest, because it is clear from the accordance of the two sets of results given in Table I. that the analytical method must give a tolerably good agreement with the observed curve, and it might be doubted whether a more precise fitting would be worth while in view of the presumed systematic errors of the individual groups.

The result shows that the systematic errors are so small that they do not neutralise the advantage of the slightly improved fit.

The point on which Smart differs most from previous determinations of the stream constants is in his high declination for the Stream II. apex. This seems to be definitely a result from the proper motions and not due to any kind of bias in the analyses, since the purely automatic method gives a still higher declination.

4. I may perhaps be allowed to add what I think are the general conclusions to be drawn from the important addition to our knowledge of star-streaming among fainter stars resulting from Dr. Smart's investigation, which rests on the long-continued and accurate series of measurements made by himself and Mr. Green. I think that the only determination of a similar kind is that made last year by Bruggencate, referred to in Smart's paper. Investigations of faint stars of *large* proper motion come under a different category, since they select the nearer stars especially and do not bear directly on the extension of star-streaming to great distances.

In order to compare the results with those for the bright stars we naturally select Boss's catalogue for comparison.* The Cambridge proper motions are smaller on account of the greater distance of the stars, but the accidental error is also considerably smaller. Moreover, the photographic proper motions are free from the systematic errors of the meridian catalogues and uncertainties of the precession, and their own systematic errors are likely to be smaller. Thus the general accuracy in θ is likely to be about the same. This is confirmed by the analytical results. Smart's mean probable error for X, Y, Z is ± 0.33 , and the probable error of my determination from Boss's catalogue was ± 0.24 ; the difference just corresponds to the greater number of stars employed in the latter investigation (6000 against 3000). We emphasise this similarity because the values of hV are decreased by a large probable error, so that they are not properly comparable unless the accuracy of the determinations of θ is the same.

The striking point is that both for solar motion and for star-streaming the bright and faint stars give the same values of hV , viz. :

Solar motion : Cambridge 0.881, Boss 0.908.

Star-streaming : Cambridge 1.840, Boss 1.868.

In going to a limit about 5 magnitudes fainter we must necessarily include stars which are on the average intrinsically fainter. It is generally believed that the peculiar velocities of these are larger, and that the solar motion is larger owing to the asymmetry of high-velocity stars. But it seems unlikely that peculiar velocity, solar motion, and differential stream velocity would all increase in the same ratio; and the result, which seems entirely trustworthy, is difficult to interpret.

In Boss's catalogue the stars were divided between the two streams in the ratio 3:2; the Cambridge stars are nearly evenly divided. The increase in the proportion of Stream II. with increasing faintness has long been suspected; but it was difficult to discriminate it from the effect of a greater accidental error affecting the proper motions of

* *Monthly Notices*, 71, 4.

fainter stars. The increase now appears to be satisfactorily established; it is fairly conspicuous when we compare the polar diagrams for the two sets of proper motions. This increased proportion of Stream II. is reflected in the higher declination of the solar apex ($+43^\circ$ compared with $+36^\circ$).

There is a change of about 8° in the position of the vertex, which seems large enough to be significant—especially when it is remembered that the vertex (unlike the solar apex) is not much affected by systematic errors in the proper motions. We do not know whether this displacement should be connected with the intrinsic faintness or more distant distribution of the stars. It seems to be in the wrong direction for an effect of Oort's high-velocity stars. I am inclined to believe that there is among the nearer (and brighter) stars an abnormal group with a vertex of low declination. This is indicated in Eddington and Hartley's investigation * of radial velocities, where the G stars give a very discordant vertex. The advantage of an investigation of the faint stars is that it covers a wide expanse and minimises local disturbances, so that the higher vertex found by Smart is likely to be more typical of stellar motions in general.

It should be added, finally, that the Cambridge proper motions have the disadvantage of uneven distribution over the sky, and there is a danger that the results may not be fairly representative of the sky as a whole. This danger is, however, minimised by the fact that they include stars extending over a great distance, so that even a single region comprises widely separated parts of the stellar universe.

Trepidation. By J. K. Fotheringham, D.Litt.

1. *Historical Retrospect.*—In 1878 Newcomb † announced the discovery of a fluctuation in the motion of the Moon, for which he was unable to provide a gravitational explanation, and which he represented by a sine term with a period of 273 years and an amplitude of $15''.5$. In 1904 ‡ Dr. Cowell showed that in addition to the major fluctuation there were minor fluctuations which he represented by two terms having periods of 66 and 42 years and coefficients of $2''.35$ and $0''.73$ respectively. Various attempts have since been made with differing degrees of success to evaluate these fluctuations empirically, but no gravitational explanation has been found as yet.

In 1914 Dr. Brown, in an address to the British Association, § suggested a similarity between the minor fluctuations in the Moon's motion and the deviation of the observed from the computed places of the Sun and Mercury. In the case of the Sun he found, what is now recognised to

* *Monthly Notices*, **75**, 526.

† *Researches on the Motion of the Moon*, part i.

‡ *M.N.*, **65**, 34-53.

§ *Report of the British Association for the Advancement of Science*, 1914, pp. 319-21.

be the reverse of the fact, that the minor fluctuations in the Moon's longitude corresponded to a fluctuation in the opposite direction in the Sun's longitude. The fluctuation in the longitude of Mercury was supposed to be in the same direction as the minor fluctuations in the longitude of the Moon. But it may be doubted whether, standing alone, the similarity would have been sufficient to warrant any conclusion. Dr. Brown was not so rash as to commit himself to a theory which would explain the supposed phenomena, but he expressed his opinion in the words: "We must therefore look for some kind of a surge spreading through the solar system and affecting planets and satellites the same way but to different degrees." He suggested with some diffidence that variations exist in the intensity of the magnetic field of the Sun, and are communicated to the magnetic fields of the other bodies of the solar system. These variations would, he thought, lead us to expect fluctuations having the same period, and of the same or opposite phase but differing in magnitude.

A theory which has met with far more favour is one which had been advanced to explain the secular acceleration and fluctuation in the Moon's motion, viz. that the fluctuations in longitude are the apparent effect of fluctuations in the Earth's rate of rotation. This theory gained in plausibility when Mr. Glauert in 1915* showed that the fluctuations in the Sun's longitude, which he determined separately from observations of the Sun, Venus, and Mercury, were not merely similar to, but in the same direction as, the minor fluctuations in the Moon's longitude. He also found that in the two intervals, 1866-75 and 1898-1909, the change in the deviation of Sun and Moon respectively was in the mean ratio of 1 to 7.2, but that the change in the deviation in the Sun's longitude from 1898 to 1909, when reduced to a mean value in which the observations made by different observers were treated separately, stood to the change in the deviation in the Moon's longitude between those dates in the ratio of 1 to 4.5, differing very slightly from the theoretical ratio of 1 to 4.8, which, according to his study, would hold if the change of momentum in the Earth were compensated by a changed orbital velocity of the Moon. He did not venture to assert this complete compensation on the strength of twelve years of observation, but he was content to hold that a partial or complete compensation existed. In a later paper in the same year,† Mr. Glauert showed that there was a general resemblance between the fluctuations in the heliocentric longitudes of Venus and the Earth, and that these fluctuations were approximately in the same ratio as the mean motions of the two planets.

In 1916 Dr. Ross contributed to the *Astronomical Journal* ‡ a paper on the Sun's Mean Longitude, which included a comparison of the fluctuations of the Sun, as separately determined from observations of the Sun and of Mars, with the fluctuations of Mars and the minor fluctuations of the Moon. These were illustrated by diagrams. For purposes of comparison the minor fluctuations of the Moon were reduced to one-fifth, but a study of his diagram will show that a further reduction would appear to be necessary. Dr. Ross concluded that the correlation

* *M.N.*, **75**, 489-95.

† *Ibid.*, **75**, 685-7.

‡ Vol. 29, pp. 152-6.

between the deviations of Sun and Moon was well marked since 1830, but that from 1750 to 1830 no connection whatever was found. His diagrams showed good agreement throughout the whole interval from 1750 to 1912 between the Sun's fluctuation as determined from observations of the Sun and Mars respectively, and close similarity between the fluctuation of Mars and that of the Sun as deduced from observations of Mars. The fluctuation of Mars was found to be perceptibly smaller than that of the Sun. He raised the question whether the major fluctuation in the Moon's longitude had its counterpart in the Sun's, but stated that positive affirmation or denial of this important question is not now possible, because it is entangled with the minor fluctuations and with the secular acceleration of the Sun's mean motion. He discussed the question of a secular acceleration of the Sun, and decided that it is quite unlikely that such an acceleration now exists.* He considered that the discrepancy between lunar and solar fluctuations before 1830 left it open to question whether the fluctuations are due entirely to changes in the Earth's rotation.

In Circular No. 65, 1925 November 14, of the Union Observatory, Johannesburg, Dr. Innes discussed the transits of Mercury, 1677-1924. He endeavoured to obtain from them evidence as to the variability of the rotation of the Earth, and for that purpose compared them with the occultations of the Moon discussed by Newcomb in his *Researches on the Motion of the Moon*, part ii., and with the eclipses of Jupiter's satellites I. and II. The discussion seems to me to be sound as a means of determining the extent to which the difference between the longitudes of Mercury and the Sun has deviated from its tabular value. If the Sun's deviation is independently determined, this should give us the deviation of Mercury. The observations of Jupiter's satellites as discussed by him do not seem to me to prove more than that the motion of those satellites exceeds the motion of Jupiter by more than the tabular difference. Dr. Innes rather assumes than proves that the fluctuations are connected with the Earth's rotation.

Dr. Brown has published in the *Transactions of the Astronomical Observatory of Yale University*, vol. iii., part vi., a paper entitled "The Evidence for Changes in the Rate of Rotation of the Earth and their Geophysical Consequences, with a Summary and Discussion of the Deviations of the Moon and Sun from their Gravitational Orbits." In this paper he tabulates in very convenient form the deviation of both bodies from their mean places. Dr. Brown departs here from the example which he had set in 1914, and which had been followed by all subsequent inquirers except Dr. Innes, in that he uses the total

* Dr. Ross examined for a term of $+3''.5T^2$, where T represents the time expressed in Julian centuries. This was roughly a mean between Dr. Cowell's value of $+4''.1$ from ancient solar eclipses and $+3''.2$ which I had found by comparing my result from ancient occultations with the Greek lunar eclipses, as reduced by Newcomb with Newcomb's weights. I subsequently adopted the value $+1''.5$, which I obtained from the ancient solar eclipses, and I found that my own study did not confirm Newcomb's solution of the lunar eclipses. Dr. Brown has requested me to correct his error in supposing that Dr. Ross had misunderstood my method of expressing the acceleration. See Dr. Brown's paper in the *Yale Transactions*, p. 221.

fluctuation of the Moon instead of the minor fluctuations only. He assumes that the fluctuations are in the same ratio (1 to 13.3) as the mean motions, and that the Sun is subject to an acceleration, yielding a term of $+1''.00T^2$. This was the first investigation of the fluctuations in which a secular acceleration of the Sun was assumed, though, as has been seen, the possibility of such an acceleration had been examined by Dr. Ross. The paper is largely occupied with a discussion of possible causes of a fluctuation in the Earth's rotation. Dr. Brown now regards this as the explanation of the apparent fluctuations in longitude, and is inclined to favour the hypothesis of deep-seated oscillations producing a widespread raising and lowering of the crust extending at least over the greater portion, if not over the whole, of the Earth's surface.

I have also had the privilege of reading rapidly a paper, still unpublished, on the Rotation of the Earth, contributed by Dr. Jones to *Monthly Notices* for 1926 November. My work is independent of his paper, which will, I hope, prove valuable to future inquirers.

2. *Longitude of the Sun.*—It has for some time been my conviction that we should gain a clearer insight into the relations of the fluctuations in the longitudes of different bodies if we were first to make independent determinations of their respective mean places and mean motions over the same range of years with the same distribution of weights and with the same theoretical assumptions. Discussions hitherto have referred the fluctuations to standard determinations of longitude which are not strictly comparable because they fail to fulfil these conditions, or, as in the case of Dr. Brown's most recent study, have, for one of the two bodies compared, proceeded on unverified assumptions as to the ratio of solar and lunar accelerations and fluctuations. In one or two papers the possibility of a correction to the mean motion so as to improve the agreement between the fluctuations has been considered, but no attempt has been made to refer the fluctuations to elements which are at the same time comparable and independent. The present paper owes its origin to a desire to make trial of this new method of approach.

The Sun and Moon are the two bodies in respect of which we possess the best digested material for comparison, and it is not the least of Dr. Brown's merits that he has exhibited very fully in one paper the discordances between the observed and tabular places of both bodies. I therefore decided to use his material to recompute the epochs and mean motions of both bodies over the years 1843–1925, ignoring all empirical terms. I decided to use the Greenwich meridian observations only, for both bodies, thus excluding from the problem any systematic differences between different kinds of observations or between different observatories. For the Moon Dr. Brown uses the mean motion and acceleration found by me in my "Solution of Ancient Eclipses of the Sun,"* but for the epoch he introduces a preliminary correction of $-0''.16$ made by Dyson and Crommelin in 1923.† The gravitational terms are taken from Dr. Brown's theory. In order to reduce these terms to pure theory, I decided to remove the empirical acceleration of the Moon. By an oversight I adopted $+6''.12T^2$ instead of Dr. Brown's value, $+6''.03T^2$.

* *M.N.*, 81 (1920), 125.

† *Ibid.*, 83, 361.

for the theoretical term at epoch 1884.5. The difference is unimportant, and its effect is eliminated in the final least-squares solution. For the Sun Dr. Brown refers all discordances to Newcomb's tabular places, which do not include any empirical term. The Sun's longitude is determined throughout from Right Ascensions only. For both bodies the observed places are reduced to the equinox of Newcomb's system N_1 , and the tabular places are deemed to refer to the same equinox.

Taking Dr. Brown's grouping of the solar observations, which as far as 1912 are extracted from Dr. Ross's paper, as standard, I formed, from the lunar discordances given by Dr. Brown, means for groups corresponding to the solar groups. Before 1850 this involved a little interpolation, since the periods used by Dr. Brown for the Moon could not be combined so as to coincide exactly with his groups for the Sun. The groups were weighted according to the number of years which they contained, and separate least-squares analyses were conducted for the Sun and Moon respectively in order to find their mean places and mean motions at the mid-epoch 1884.5. The resultant corrections were :

For the Moon	$\Delta L = -6''.73 - 23''.34T$	(1)
For the Sun	$\Delta L' = +0''.35 + 1''.39T$	(2)

where T is the interval in Julian centuries elapsed since 1884.5. The residuals obtained by applying these corrections to the tabular places were adopted as the basis of comparison of the two bodies. In each case they represent the total deviation of the body from its place and motion as they existed in 1884.5, subject only to known gravitational terms.

In the following tables each group receives a number determined by its place in the final solution. Under a there is given for the Moon, with sign reversed, the total fluctuation, meaned from Dr. Brown's table, where it is called "Th.-G." Under b is given $4''.7$ multiplied by the square of the number of centuries elapsed at the middle of each group from the epoch 1884.5. c for the Moon is the sum of a and b , and represents the deviation from gravitational place before recomputation for epoch. For the Sun, c is taken direct, with sign reversed, from Dr. Brown's table, where it is called "Th.-O." d is $= +6''.73 + 23''.34T$ for the Moon and $-0''.35 - 1''.39T$ for the Sun. It represents the reduction to mean place and mean motion as computed from the observations under discussion. fm and fs are each the sum of c and d , and represent for Moon and Sun respectively the total deviation from gravitational place as computed from the observations under discussion.

Graphs were drawn to represent the deviations shown in the two columns fm and fs , and it was immediately apparent that they followed very similar curves and approximated to a constant ratio. The arithmetical mean ratio was found to be 1 to 9.4, but when weighted by the square of the Sun's deviation multiplied by the tabular weight, it was reduced to 1 to 6.9. A graph was drawn in which the Sun's deviation was magnified seven times, and it was clear that only a slight adjustment was required to bring the graphs for Sun and Moon into agreement.

From this it was inferred that within the range of years examined,

Observed—Tabular Longitudes.

Moon.

No.	Wt.	Date.	a.	b.	c.	d.	fm.
5	I	1843-46	+ 3'71	+ 0'73	+ 4'44	- 2'49	+ 1'95
6	I	47-50	+ 3'51	+ 0'59	+ 4'10	- 1'56	+ 2'54
7	I	51-54	+ 3'58	+ 0'47	+ 4'05	- 0'62	+ 3'43
8	I	55-58	+ 2'91	+ 0'36	+ 3'27	+ 0'31	+ 3'58
9	I	59-62	+ 1'49	+ 0'26	+ 1'75	+ 1'25	+ 3'00
10	I	63-66	+ 0'36	+ 0'18	+ 0'54	+ 2'18	+ 2'72
11	I	67-70	- 2'73	+ 0'11	- 2'62	+ 3'11	+ 0'49
12	I	71-74	- 6'09	+ 0'06	- 6'03	+ 4'05	- 1'98
13	I	75-78	- 8'97	+ 0'03	- 8'94	+ 4'98	- 3'96
14	I	79-82	- 10'01	0'00	- 10'01	+ 5'91	- 4'10
15	I·5	83-88	- 12'09	0'00	- 12'09	+ 7'08	- 5'01
16	I	89-92	- 13'80	+ 0'02	- 13'78	+ 8'25	- 5'53
17	I	93-96	- 15'44	+ 0'05	- 15'39	+ 9'18	- 6'21
18	I	97-1900	- 15'34	+ 0'10	- 15'24	+ 10'11	- 5'13
19	I	1901-04	- 14'09	+ 0'16	- 13'93	+ 11'05	- 2'88
20	I	05-08	- 12'59	+ 0'24	- 12'35	+ 11'98	- 0'37
21	I	09-12	- 11'56	+ 0'33	- 11'23	+ 12'92	+ 1'69
22	I	13-16	- 9'82	+ 0'44	- 9'38	+ 13'85	+ 4'47
23	I	17-20	- 9'40	+ 0'56	- 8'84	+ 14'78	+ 5'94
24	0·75	21-23	- 9'77	+ 0'68	- 9'09	+ 15'60	+ 6'51
25	0·5	24-25	- 10'97	+ 0'77	- 10'20	+ 16'18	+ 5'98

Sun.

No.	Wt.	Date.	c.	d.	fs.
5	I	1843-46	+ 0'16	+ 0'20	+ 0'04
6	I	47-50	+ 0'10	+ 0'14	+ 0'24
7	I	51-54	+ 0'29	+ 0'09	+ 0'38
8	I	55-58	+ 0'08	+ 0'03	+ 0'11
9	I	59-62	+ 0'17	- 0'02	+ 0'15
10	I	63-66	+ 0'48	- 0'08	+ 0'40
11	I	67-70	+ 0'45	- 0'13	+ 0'32
12	I	71-74	+ 0'06	- 0'19	- 0'13
13	I	75-78	+ 0'09	- 0'25	- 0'16
14	I	79-82	+ 0'35	- 0'30	+ 0'05
15	I·5	83-88	- 0'13	- 0'37	- 0'50
16	I	89-92	- 0'15	- 0'44	- 0'59
17	I	93-96	- 0'27	- 0'50	- 0'77
18	I	97-1900	- 0'33	- 0'55	- 0'88
19	I	1901-04	+ 0'04	- 0'61	- 0'57
20	I	05-08	+ 0'49	- 0'67	- 0'17
21	I	09-12	+ 0'88	- 0'72	+ 0'16
22	I	13-16	+ 1'21	- 0'77	+ 0'44
23	I	17-20	+ 1'59	- 0'83	+ 0'76
24	0·75	21-23	+ 1'63	- 0'88	+ 0'75
25	0·5	24-25	+ 1'63	- 1'01	+ 0'72

the total deviations of Sun and Moon, including empirical acceleration, major fluctuation, and minor fluctuations, approximated to a constant ratio.

The examination was then carried back to 1750, the same corrections being applied as in the earlier part of the investigation. In the following tables *a* and *b* have the same meanings as before. It was not considered necessary to compute *c* for the Moon. *c* for the Sun and *d* for both bodies have the same meanings as before. *fm* is the sum of *a*, *b*, and *d*, while *fs*, as before, is the sum of *c* and *d*. *fm* and *fs* represent the total deviation of the respective bodies from their theoretical positions as computed, using as before the equations (1) and (2).

Observed—Tabular Longitudes.

No.	Date.	Moon.				Sun.		
		<i>a.</i>	<i>b.</i>	<i>d.</i>	<i>fm.</i>	<i>c.</i>	<i>d.</i>	<i>fs.</i>
1	1750-62	+ 9 ⁰ 05	+ 7 ⁰ 70	- 23 ⁰ 15	- 6 ⁰ 40	- 0 ⁰ 63	+ 1 ⁰ 43	+ 0 ⁰ 80
2	65-71	+ 12 ⁰ 00	+ 6 ⁰ 33	- 20 ⁰ 34	- 2 ⁰ 01	- 0 ⁰ 28	+ 1 ⁰ 26	+ 0 ⁰ 98
3	72-78	+ 13 ⁰ 10	+ 5 ⁰ 58	- 18 ⁰ 71	- 0 ⁰ 03	- 0 ⁰ 35	+ 1 ⁰ 17	+ 0 ⁰ 82
	79-85	+ 14 ⁰ 87	+ 4 ⁰ 89	- 17 ⁰ 08	+ 2 ⁰ 68	+ 1 ⁰ 94	+ 1 ⁰ 07	+ 3 ⁰ 01
	86-92	+ 15 ⁰ 62	+ 4 ⁰ 24	- 14 ⁰ 44	+ 4 ⁰ 42	+ 0 ⁰ 69	+ 0 ⁰ 97	+ 1 ⁰ 66
	93-97	+ 14 ⁰ 44	+ 3 ⁰ 72	- 14 ⁰ 04	+ 4 ⁰ 12	+ 1 ⁰ 13	+ 0 ⁰ 89	+ 2 ⁰ 02
	98-1802	+ 13 ⁰ 11	+ 3 ⁰ 32	- 12 ⁰ 88	+ 3 ⁰ 55	+ 0 ⁰ 3	+ 0 ⁰ 82	+ 1 ⁰ 12
	1803-06	+ 11 ⁰ 53	+ 2 ⁰ 97	- 11 ⁰ 83	+ 2 ⁰ 67	- 0 ⁰ 4	+ 0 ⁰ 76	+ 0 ⁰ 36
	07-10	+ 11 ⁰ 65	+ 2 ⁰ 68	- 10 ⁰ 89	+ 3 ⁰ 44	- 1 ⁰ 0	+ 0 ⁰ 70	- 0 ⁰ 30
	11-14	+ 9 ⁰ 07	+ 2 ⁰ 40	- 9 ⁰ 96	+ 1 ⁰ 51	- 3 ⁰ 98	+ 0 ⁰ 64	- 3 ⁰ 34
	15-18	+ 8 ⁰ 48	+ 2 ⁰ 14	- 9 ⁰ 02	+ 1 ⁰ 60	- 2 ⁰ 19	+ 0 ⁰ 59	- 1 ⁰ 60
	19-22	+ 7 ⁰ 15	+ 1 ⁰ 89	- 8 ⁰ 09	+ 0 ⁰ 95	- 1 ⁰ 38	+ 0 ⁰ 53	- 0 ⁰ 85
	23-26	+ 7 ⁰ 98	+ 1 ⁰ 66	- 7 ⁰ 16	+ 2 ⁰ 48	- 1 ⁰ 91	+ 0 ⁰ 48	- 1 ⁰ 43
	27-30	+ 7 ⁰ 07	+ 1 ⁰ 45	- 6 ⁰ 22	+ 2 ⁰ 30	- 0 ⁰ 77	+ 0 ⁰ 42	- 0 ⁰ 35
	31-34	+ 4 ⁰ 96	+ 1 ⁰ 25	- 5 ⁰ 29	+ 0 ⁰ 92	+ 0 ⁰ 64	+ 0 ⁰ 37	+ 1 ⁰ 01
	35-38	+ 3 ⁰ 96	+ 1 ⁰ 06	- 4 ⁰ 36	+ 0 ⁰ 66	+ 1 ⁰ 02	+ 0 ⁰ 31	+ 1 ⁰ 33
4	39-42	+ 3 ⁰ 57	+ 0 ⁰ 89	- 3 ⁰ 42	+ 0 ⁰ 94	+ 0 ⁰ 27	+ 0 ⁰ 25	+ 0 ⁰ 52

Graphs were drawn to represent the deviations of the two bodies for the years under review. It was anticipated that, if the constant ratio did not hold, the two graphs would eventually tend to run in different directions. It was found, however, that for the delicate purpose of the present investigation the solar observations from 1779 to 1838 were useless. Although the solar and lunar curves present a vague similarity, the solar observations are marked by violent fluctuations from group to group, of which there is no example earlier or later. The excellence of the observations of the Bradley period is well known. Not only do the groups from 1750 to 1778 give consistent results for the Sun's Right Ascension, but Newcomb in his *Astronomical Constants*

(1895), p. 22, weighting by internal discordances, has given these groups far higher weight than any later groups before 1815. It was therefore, at first sight, disappointing to find that the graphs for Sun and Moon for these years were pointing in widely different directions. If the Sun's deviation is multiplied by 7, we get a discrepancy of as much as $12''$ for the first group. From this it seems to follow that some part of the Sun's deviation is not in a ratio resembling 1 to 7 to the Moon's deviation, and that this part, while not very apparent within 45 years of the epoch, has a marked effect at a distance of 130 years. It was natural to think that this part might be the secular acceleration. In my "Solution of Ancient Eclipses of the Sun" I had adopted $+1''.5T^2$ as the best value for the solar acceleration term given by the ancient observations. This value stands to the empirical lunar acceleration term assumed in this paper in the ratio of 1 to 3.1, and I therefore proposed to consider the effect of inserting its value as determined from the ancient observations. It seemed to me that the mean motion of the Sun determined in the earlier part of this discussion would probably involve the substitution of $+1''.6$ for $+1''.5$ in the acceleration term as determined from the ancient observations. Of this quantity I supposed that about one-seventh of the empirical lunar acceleration, or approximately $+0''.7T^2$, was included in the constant ratio, and that the adoption of the value of $+1''.6$ for the solar acceleration would require a correction of $-(1''.6-0''.7)T^2 = -0''.9T^2 = -1''.5$ to the Sun's deviation at epoch 1756.5. This would convert the deviation to $-0''.7$, approximately one-ninth of the Moon's deviation. It appeared, therefore, that the whole series of observations might be satisfied within the range of observational error on the assumption that the accelerations determined from the ancient observations held good, and that the total fluctuations of both bodies preserved a fixed ratio. But it must be remembered that from 1779 to 1838 almost anything was within the range of observational error.

It is, however, unnecessary to introduce an acceleration determined from outside the Greenwich observations in order to test the agreement of the two series of observations.

We can express the correction necessary to bring each observed solar longitude into agreement with the contemporary lunar longitude by the formula

$$\alpha + \beta T + \gamma T^2 + \rho f m = f s.$$

α and β are here corrections to be applied to the Sun's mean place and mean motion respectively at epoch 1884.5. ρ is the constant ratio of the solar to the lunar fluctuation, and γ is the excess of the solar acceleration over the empirical lunar acceleration multiplied by ρ .

Putting $x = \alpha$, $y = \beta$, $z = \gamma$, $u = 5\rho$, the equations of condition shown in the following table were formed. The weights attached to each equation are taken from the sixth column on p. 22 of Newcomb's *Astronomical Constants* so far as it extends. To mark the increasing accuracy of the observations, I decided to attach weight 4 to each group from 1893 to 1912, and weight 5 to Group 23, the only 4-year group, throughout the whole of which the travelling micrometer wire was in use. Group 22, during half of which this wire was used, received weight

4.5, and Groups 24 and 25, containing 3 and 2 years respectively, receive weights 4 and 2.5. The observations from 1779 to 1838 inclusive were not used in the analysis.

Analysis of the Sun's Longitude, 1750-1925.

Equations of Condition.

No.	Wt.	x .	y .	z .	u .	
1	1.5	+1	-1.28	+1.638	-1.280	= +0.80
2	0.5	+1	-1.16	+1.346	-0.402	+0.98
3	0.5	+1	-1.09	+1.188	-0.006	+0.82
4	1	+1	-0.435	+0.189	+0.188	+0.52
5	2	+1	-0.395	+0.156	+0.390	+0.04
6	2	+1	-0.355	+0.126	+0.508	+0.24
7	3	+1	-0.315	+0.099	+0.686	+0.38
8	3	+1	-0.275	+0.076	+0.716	+0.11
9	3	+1	-0.235	+0.055	+0.600	+0.15
10	3	+1	-0.195	+0.038	+0.544	+0.40
11	3	+1	-0.155	+0.024	+0.098	+0.32
12	3	+1	-0.115	+0.013	-0.396	-0.13
13	3	+1	-0.075	+0.006	-0.792	-0.16
14	3	+1	-0.035	+0.001	-0.820	+0.05
15	3	+1	+0.015	0.000	-1.002	-0.50
16	2	+1	+0.065	+0.004	-1.106	-0.59
17	4	+1	+0.105	+0.011	-1.242	-0.77
18	4	+1	+0.145	+0.021	-1.026	-0.88
19	4	+1	+0.185	+0.034	-0.576	-0.57
20	4	+1	+0.225	+0.051	0.074	-0.17
21	4	+1	+0.265	+0.070	+0.338	+0.16
22	4.5	+1	+0.305	+0.093	+0.894	+0.44
23	5	+1	+0.315	+0.119	+1.188	+0.76
24	4	+1	+0.380	+0.144	+1.302	+0.75
25	2.5	+1	+0.405	+0.164	+1.196	+0.72

These give the following normal equations:—

x .	y .	z .	u .	
72.5	+0.321	+8.179	+4.391	= + 5.52
0.321	+8.179	-4.228	+5.35	= - 1.42
8.179	-4.228	+6.117	-0.47	= + 4.70
4.391	+5.35	-0.47	+51.366	= + 25.27

From these it follows that the correction to the Sun's longitude as obtained in (2) is

$$\Delta L' = -0''.046 - 0''.090T + 0''.808T^2 + 0.1025fm \quad (1884.5)^* \quad (3)$$

$$\pm''\cdot033 \quad \pm''\cdot12 \quad \pm''\cdot14 \quad \pm''\cdot0072$$

It will be remembered that fm includes not only the total fluctuation of the Moon, but also the empirical acceleration, or, more accurately, the amount by which the total acceleration at epoch 1884.5 exceeded $6''.12$ per century. Its coefficient, 0.1025 ± 0.0072 , or $1/(9.76 \pm 0.69)$, expresses the constant ratio of the solar to the lunar fluctuation.

* But see note at end of paper.

In the next table fs has the same meaning as above, viz. the observed excess of the Sun's longitude over that resulting from (2), a is $+0''.05$, b is $+0''.09T$, c is $-0''.808T^2$, and d is $-0.1025 fm$. The figures in the column "Res." are the sum of fs , a , b , c , and d , and give the residuals left after applying the final correction $\Delta L'$. These have been computed not only for the observations used in the analysis, but for each group from 1750 to 1925. They represent the excess of the observed over the computed longitude of the Sun after applying formula (3).

Comparison with Formula (3).

No.	Date.	fs .	a .	b .	c .	d .	Res.
1	1750-62	$+0''.80$	$+0''.05$	$-0''.12$	$-1''.32$	$+0''.66$	$+0''.07$
2	65-71	$+0.98$	$+0.05$	-0.10	-1.09	$+0.21$	$+0.05$
3	72-78	$+0.82$	$+0.05$	-0.10	-0.96	0.00	-0.19
	79-85	$+3.01$	$+0.05$	-0.09	-0.84	-0.27	$+1.86$
	86-92	$+1.66$	$+0.05$	-0.09	-0.73	-0.45	$+0.44$
	93-97	$+2.02$	$+0.05$	-0.08	-0.64	-0.42	$+0.93$
	98-1802	$+1.12$	$+0.05$	-0.08	-0.57	-0.36	$+0.16$
	1803-06	$+0.36$	$+0.05$	-0.07	-0.51	-0.27	-0.44
	07-10	-0.30	$+0.05$	-0.07	-0.46	-0.35	-1.13
	11-14	-3.34	$+0.05$	-0.06	-0.41	-0.15	-3.91
	15-18	-1.60	$+0.05$	-0.06	-0.37	-0.16	-2.14
	19-22	-0.85	$+0.05$	-0.06	-0.33	-0.10	-1.29
	23-26	-1.43	$+0.05$	-0.05	-0.29	-0.25	-1.97
	27-30	-0.35	$+0.05$	-0.05	-0.25	-0.24	-0.84
	31-34	$+1.01$	$+0.05$	-0.05	-0.21	-0.09	-0.71
	35-38	$+1.33$	$+0.05$	-0.04	-0.18	-0.07	$+1.09$
4	39-42	$+0.52$	$+0.05$	-0.04	-0.15	-0.10	$+0.28$
5	43-46	$+0.04$	$+0.05$	-0.04	-0.13	-0.20	-0.28
6	47-50	$+0.24$	$+0.05$	-0.03	-0.10	-0.26	-0.10
7	51-54	$+0.38$	$+0.05$	-0.03	-0.08	-0.35	-0.03
8	55-58	$+0.11$	$+0.05$	-0.02	-0.06	-0.37	-0.29
9	59-62	$+0.15$	$+0.05$	-0.02	-0.04	-0.31	-0.17
10	63-66	$+0.40$	$+0.05$	-0.02	-0.03	-0.28	$+0.12$
11	67-70	$+0.32$	$+0.05$	-0.01	-0.02	-0.05	$+0.29$
12	71-74	-0.13	$+0.05$	-0.01	-0.01	$+0.20$	$+0.10$
13	75-78	-0.16	$+0.05$	-0.01	0.00	$+0.41$	$+0.29$
14	79-82	$+0.05$	$+0.05$	0.00	0.00	$+0.42$	$+0.52$
15	83-88	-0.50	$+0.05$	0.00	0.00	$+0.51$	$+0.06$
16	89-92	-0.59	$+0.05$	$+0.01$	0.00	$+0.57$	$+0.04$
17	93-96	-0.77	$+0.05$	$+0.01$	-0.01	$+0.64$	-0.08
18	97-1900	-0.88	$+0.05$	$+0.01$	-0.02	$+0.53$	-0.31
19	1901-04	-0.57	$+0.05$	$+0.02$	-0.03	$+0.30$	-0.23
20	05-08	-0.17	$+0.05$	$+0.02$	-0.04	$+0.04$	-0.10
21	09-12	$+0.16$	$+0.05$	$+0.02$	-0.06	-0.17	0.00
22	13-16	$+0.44$	$+0.05$	$+0.03$	-0.08	-0.46	-0.02
23	17-20	$+0.76$	$+0.05$	$+0.03$	-0.10	-0.61	$+0.13$
24	21-23	$+0.75$	$+0.05$	$+0.04$	-0.12	-0.67	$+0.05$
25	24-25	$+0.72$	$+0.05$	$+0.04$	-0.13	-0.61	$+0.07$

It will be observed that the least-squares solution, although employing four unknowns, has done no violence to the Sun's place and mean motion as independently determined for the epoch 1884.5. The corrections to these two elements are trifling. The two orbits are practically brought into agreement by correcting the solar elements for two quantities only, a constant acceleration and a fluctuation which bears a constant ratio to the lunar fluctuation. It will be seen later that this constant acceleration is in good agreement with the acceleration deduced from the ancient observations. The small size of the residuals is also worthy of attention. The improvement beginning in 1839 is doubtless due to Airy. Within the two ranges of years in which the observed longitudes are not hopelessly in error, *i.e.* from 1750 to 1778 and from 1839 to 1925, only one residual amounts to a third of a second of arc. It would be possible to read into these residuals a period of about 50 years' duration, but it is more likely that any systematic variation in the residuals belongs rather to the history of the instruments and observers at Greenwich than to the motion of the Earth.

Incidentally these residuals show that Mr. Cullen* was in error in supposing that the introduction of the travelling micrometer wire in 1915 produced a discontinuity in the Greenwich observations of Right Ascension, amounting to 0".60 or thereabouts. A reference to the *Greenwich Observations*, 1914, A ix, compared with the Introduction to the *First Greenwich Catalogue of Stars for 1925.0*, will show that at that time there was a steady annual increase in the excess of the observed over the tabular Right Ascension of the Sun, which, as the table on p. 219 of Dr. Brown's paper shows, was common to the Moon also. In the case of the Moon this excess appears to have reached its maximum in 1917, and, after remaining stationary for a few years, has now begun to diminish. The comparison between the two Greenwich volumes shows also that of the four Greenwich observers of 1914, the observations of one only, Mr. Witchell, exhibit a greater increase in Right Ascension between that year and 1915 or 1916 than we should expect from the annual advance of the Sun. But Mr. Witchell's Right Ascensions were in 1914 markedly less than those of any other observer, and he made about a third of the whole number of observations of the Sun in that year. In 1915, using the new method, he was in line with his colleagues. 1915 was, however, characterised by the appearance of a new observer, Dr. Jackson, who had a marked personality in the opposite direction, and in 1915 and 1916 he took the largest share in the observations of the Sun—rather less than a third in each year. Since 1916 there has been little change in the observed correction to the Sun's Right Ascension as referred to Newcomb's tables, but, as the residuals will show, the Sun has really kept pace with the Moon's fluctuation. It is possible that the change from negative to positive residuals shown in the table as we pass from 1913-6 to 1917-20 may be largely due to the change in Mr. Witchell's personality on the introduction of the travelling micrometer wire and to the appearance of Dr. Jackson as an observer. There appears to be no reason for regarding it as more important than the

* *M.N.*, 85 (1925), 1003.

changes in *personnel* made on other occasions, which I have not thought it desirable to investigate in this paper.

The combined effect of formulæ (2) and (3) is to introduce such corrections to the Sun's place as determined by Newcomb as will, on the assumption that a constant ratio exists between the solar and lunar fluctuations, reduce the solar residuals to a minimum without altering the lunar elements. These elements are, however, expressed in terms of a mean place and mean motion determined from the observations of 1843-1925. It remains to reduce these to absolute mean place and mean motion. For this purpose I will assume that the elements of the Moon's longitude found by me in my "Solution of Ancient Eclipses of the Sun" are, apart from a small correction to epoch, correct. The effect on the computed longitude of the Sun of any error in the Moon's longitude will be only one-tenth of the error in the Moon's longitude.

To reduce the Moon to mean place and mean motion as assumed in Dr. Brown's paper, it is necessary to apply ΔL as given in (1) with sign reversed and to restore the assumed secular acceleration, $+4''.70T$. To this should be added the final correction to epoch found by Dyson and Crommelin,* amounting to $+0''.09$. The total reduction for the Moon is therefore

$$+6''.82 + 23''.34T + 4''.70T^2.$$

On the assumption, which appears to be well established in this discussion, that the Sun's deviation exceeds 0.1025 of the Moon's deviation by $0''.81T^2$, we have for the corresponding reduction to be applied to the Sun's longitude,

$$\begin{aligned} & +0''.70 + 2''.39T + 0''.48T^2 \\ & -0''.05 - 0''.09T + 0''.81T^2 \\ & = +0''.65 + 2''.30T + 1''.29T^2 \quad (1884.5). \end{aligned}$$

Adding formula (2), we get as the total correction to Newcomb's longitude at epoch 1884.5,

$$+1''.00 + 3''.69T + 1''.29T^2 \quad (1884.5) \quad . \quad . \quad (4)$$

Reduced to the epochs in common use, we have as the total correction to Newcomb's mean longitude of the Sun as referred to the equinox of his system N_1 ,

$$-1''.20 + 1''.51T + 1''.29T^2 \quad (1800.0) \quad . \quad . \quad (5)$$

$$+1''.60 + 4''.09T + 1''.29T^2 \quad (1900.0)^\dagger \quad . \quad . \quad (6)$$

Attention may specially be called to the secular acceleration, now deduced for the first time from the modern observations. Newcomb's value for this term is $-0''.02T^2$, so that the final value for the coefficient now stands at $+1''.27 \pm 0''.14$. This determination is not wholly independent of the ancient observations. It is necessary to take at least one term from those observations. In this discussion it has been assumed that $+10''.80T^2$ is the total acceleration term in the Moon's motion as referred to the fixed stars at epoch 1800.0. But the best determined term is not the acceleration of either luminary, but the

* *Loc. cit.*, p. 366.

† But see note at end of paper.

difference between them. In my discussion I had found for this term a coefficient of $+9''.3$. Herr Schoch, who has made a very exhaustive study of ancient observations, finds $+9''.56$.* If the value now found for the solar acceleration is to be made to cohere with the value assumed for the lunar acceleration, the coefficient of T^2 in the difference between the longitudes of the two bodies will be $+9''.53$ at epoch 1800.0. It appears to me, therefore, that the value now obtained for the acceleration of the Sun coheres both with the adopted value for the lunar acceleration and with the best attested value for the difference between the two accelerations. It is also in excellent agreement with the value of $+1''.5$ which I had deduced for the solar acceleration from the ancient observations, and it is far more exact than any value which can be deduced from the ancient observations alone. It must be remembered that any alteration in the value found here would imply an alteration of 8.76 times as much in the difference between the two accelerations as determined from ancient observations.

The evidence from ancient observations has increased since the date of my paper, "A Solution of Ancient Eclipses of the Sun." An examination of three eclipses of the Sun described as total in the annals of the Chinese state of Lu has shown that one cannot have been total anywhere in that neighbourhood on any theory; one was total in Lu on all imaginable theories; and one was only total in that state within a narrow range of values for the elements, including both the elements proposed by me and those proposed by Herr Schoch.† Quite recently Herr Schoch has discovered that the Babylonian tablet (CBS. 11901), of the year -424, which Father Kugler‡ had regarded as an ephemeris, is really an observation tablet. This is proved not only by the phraseology, but by the close accord of the dates of appearances and disappearances of Mercury and Mars with the dates which Herr Schoch has computed on the basis of other Babylonian observations. The tablet informs us that on Tešrit 15, equivalent to October 9 Julian, the Moon was eclipsed 10 $U\check{S}$ after sunset. According to Babylonian usage this should mean that the eclipse began 40 minutes after sunset. An eclipse so close to sunset leaves little opportunity for an error in time-keeping. Herr Schoch's elements make the beginning of the eclipse fall 47 minutes after sunset. A correction of $\pm 1''T^2$ in the difference between the two longitudes would entail a correction of ∓ 16 minutes in the time of the eclipse, but it would only entail a correction of $\pm 0''.11T^2$ to the longitude of the Sun deduced in this paper. It is, however, practically impossible to increase the difference in acceleration appreciably without moving the critical solar eclipse of Hipparchus away from the Hellespont, or to reduce it without placing the Babylonian eclipse of -424 October 9 too late. I infer, therefore, that the uncertainty as to the difference between the values for the accelerations to be deduced from ancient observations is no longer sufficient to affect the probable error attached to the acceleration determined in this paper,

* In his paper *Die säkulare Acceleration des Mondes und der Sonne*, published in Berlin in 1926 December, Herr Schoch has substituted the value $+9''.60$.

† See my *Historical Eclipses* (1921), 21.

‡ *Sternkunde und Sterndienst, Ergänzungen*, II. Teil (1914), 233-42.

and the determination of that acceleration ought to carry with it the determination of the lunar acceleration. I propose, therefore, to retain $+10''.80T^2$ as the acceleration term in the sidereal acceleration of the Moon, and to adopt $+1''.27$ for the corresponding term in the acceleration of the Sun. The corrections (5) and (6) found to Newcomb's mean longitude of the Sun are, in consequence, to be treated as definitive.

In the table on p. 156 I give the total fluctuation of the Sun and Moon for the different groups, as compared with their theoretical places, assuming the mean places, mean epochs, and mean motions adopted in this discussion. In the case of the Moon, the lunar fluctuation is obtained by applying Dyson and Crommelin's uniform correction of $-0''.09$ to the values shown in column (a) of my tables of Observed—Tabular Longitudes. In the case of the Sun the fluctuation is obtained by adding the value of formula (4) with sign reversed, entered as "Reduction," to the fluctuation given in column (c) of my tables of Observed—Tabular Longitudes, which repeats with sign reversed that given by Dr. Brown in the column "Th.—O." on p. 223 of his paper.

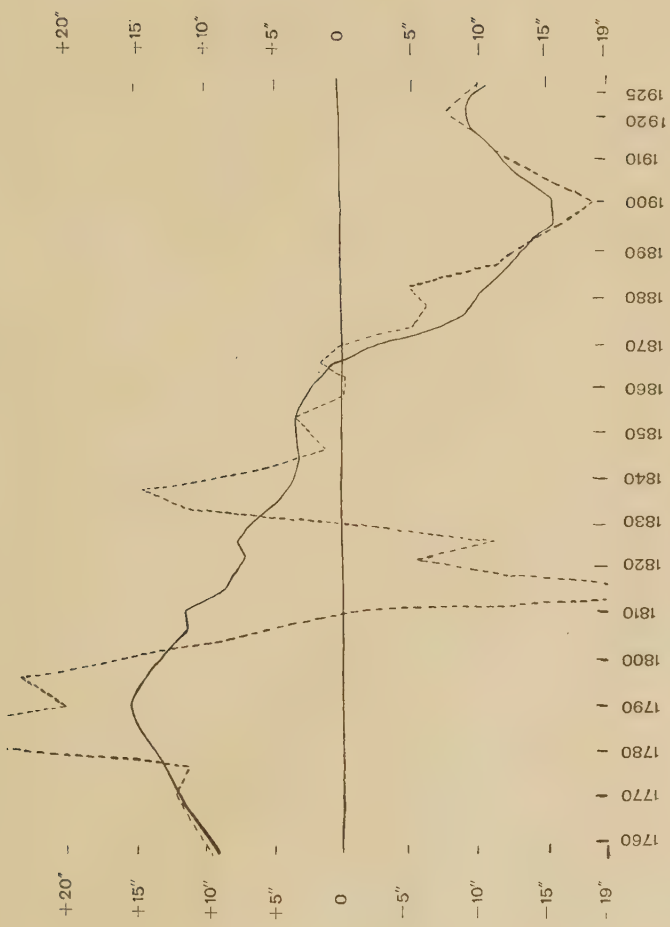
In order to test the generally accepted view that the fluctuation in the Sun's longitude should be compared not with the total fluctuation of the Moon, but with the minor fluctuations only, the Moon's motion between 1843 and 1925 was analysed on the assumption that the great empirical term as determined by Dyson and Crommelin was to be regarded as theoretical. The fluctuations were re-computed on this hypothesis and a graph was drawn. It was found that this hypothesis would give a ratio of 1 in 6.9 between the solar and lunar fluctuations, but that it would imply perceptibly larger errors than the adopted hypothesis in the recent Greenwich observations of the Sun, and it would imply a solar acceleration markedly below the value given by the ancient observations. It would, moreover, give a slope in the opposite direction to the Moon's curve from 1750 to 1778, and, as will be seen, this is quite inconsistent with the observed mass of Venus at that epoch. It gives, therefore, results decidedly inferior to those derived from the hypothesis that it is the total fluctuations of both bodies that preserve a constant ratio, and in any case the distinction between major and minor fluctuations appears to be artificial, and the close agreement of the adopted hypothesis with the observations appears to leave no room for this refinement.

Dr. Brown has called in question the value of the analysis to which Dyson and Crommelin subjected the minor fluctuation. It appears to me that some such analysis is necessary if we wish to obtain the best possible value for the great empirical term. While not pinning myself to the actual numerical expression which they have obtained, I think it important to insist on the reality of a minor periodicity of about 70 years in the fluctuation. I obtain by interpolation from the residuals given in my paper in *M.N.*, 80, 303, the following dates of maximum and minimum of the minor fluctuation, considered as a single fluctuation:—

Maximum.	Minimum.	Maximum.	Minimum.
1662 or earlier	1683.5	1774	[1804]
1709	[1726]	[1821]	1829
[1735]	1757.5	1859	1892

<i>Total Fluctuations.</i>					
No.	Date.	Sun referred to Newcomb.	Reduction.	Total Observed Fluctuation of Sun.	Total Observed Fluctuation of Moon.
1	1750-62	-0 ^u 63	+1 ^u 61	+0 ^u 98	+ 8 ^u 96
2	65-71	-0.28	+1.54	+1.26	+11.91
3	72-78	-0.35	+1.49	+1.14	+13.01
	79-85	+1.94	+1.42	+3.36	+14.78
	86-92	+0.69	+1.35	+2.04	+15.53
	93-97	+1.13	+1.26	+2.39	+14.35
	98-1802	+0.3	+1.19	+1.49	+13.02
	1803-06	-0.4	+1.11	+0.71	+11.44
	07-10	-1.0	+1.05	+0.05	+11.56
	11-14	-3.98	+0.98	-3.00	+ 8.98
	15-18	-2.19	+0.90	-1.29	+ 8.39
	19-22	-1.38	+0.82	-0.56	+ 7.06
	23-26	-1.91	+0.74	-1.17	+ 7.89
	27-30	-0.77	+0.65	-0.12	+ 6.98
	31-34	+0.64	+0.56	+1.20	+ 4.87
	35-38	+1.02	+0.46	+1.48	+ 3.87
4	39-42	+0.27	+0.37	+0.64	+ 3.48
5	43-46	-0.16	+0.26	+0.10	+ 3.62
6	47-50	+0.10	+0.15	+0.25	+ 3.42
7	51-54	+0.29	+0.03	+0.32	+ 3.49
8	55-58	+0.08	-0.09	-0.01	+ 2.82
9	59-62	+0.17	-0.20	-0.03	+ 1.40
10	63-66	+0.48	-0.33	+0.15	+ 0.27
11	67-70	+0.45	-0.46	-0.01	- 2.82
12	71-74	+0.06	-0.60	-0.54	- 6.18
13	75-78	+0.09	-0.73	-0.64	- 9.06
14	79-82	+0.35	-0.87	-0.52	-10.10
15	83-88	-0.13	-1.06	-1.19	-12.18
16	89-92	-0.15	-1.25	-1.40	-13.89
17	93-96	-0.27	-1.40	-1.67	-15.53
18	97-1900	-0.33	-1.57	-1.90	-15.43
19	1901-04	+0.04	-1.72	-1.68	-14.18
20	05-08	+0.49	-1.90	-1.41	-12.68
21	09-12	+0.88	-2.07	-1.19	-11.65
22	13-16	+1.21	-2.25	-1.04	- 9.91
23	17-20	+1.59	-2.42	-0.83	- 9.49
24	21-23	+1.63	-2.59	-0.96	- 9.86
25	24-25	+1.63	-2.70	-1.07	-11.06

These fluctuations are plotted on the accompanying diagram.



Black Line - Fluctuation of Moon.
Broken Line - Fluctuation of Sun multiplied by 9.76.

Later observations give another maximum in 1917. Here we have a period, not numerically exact, averaging 69.5 years, whether we examine the maxima or the minima. Dyson and Crommelin found by periodogram analysis of meridian observations from 1750 to 1922, for the larger of their two terms, a period of 69.6 years. My figures are based on a revision of Newcomb's occultations from 1621 to 1907, and on meridian observations from 1908 to 1918. The exact agreement in the length of the period is, doubtless, accidental, but it is equally marked in both classes of observations. It will be observed that in my own study the interval from minimum to maximum is always shorter than that from maximum to minimum, and that on two occasions the latter shows a secondary peak. Dyson and Crommelin's method of analysis does not bring this out.

3. *The Mass of Venus.*—The fluctuations in the longitudes of Sun and Moon having been determined, it remains to seek other examples of the same fluctuations.

I have not applied any accurate test to the observed longitudes of the planets, but I have succeeded in finding the same fluctuation in the mass of Venus as deduced from the inequalities which she produces in the longitudes of the Sun and Mars.

In a note on the Mass of Venus contributed to *M.N.*, 86 (1926), 298, 299, I showed that the determinations of the mass of this planet made at different epochs vary more than we should expect from their internal discordances. Classifying Newcomb's group determinations from observations of the Sun into super-groups, I found corrections for different epochs to the mass of Venus. Following Newcomb's example, I treat Leverrier's provisional value for the mass of Venus expressed in terms of the mass of the Sun, viz. $1/401847$, as unity, and express corrections as a fraction of this value.

Newcomb's determinations from the Greenwich meridian observations of the Sun when treated in this way gave the following corrections to the mass :—

1750-1792	$-.031 \pm .0023$
1793-1818	$-.042 \pm .018$
1819-1842	$+.006 \pm .012$
1843-1866	$-.021 \pm .0071$
1867-1892	$+.006 \pm .0055$

I also gave Dr. Cowell's determination from Greenwich meridian observations of the Sun, which gives

$$1864-1900 \quad +.007.$$

To these I am now able to add Dr. Jones's determination from Greenwich meridian observations of the Sun : *

$$1836-1923 \quad -.007 \pm .0019.$$

* *M.N.*, 86 (1926), 432-5.

The five separate groups for which Dr. Ross has determined the mass of Venus from observations of Mars give

1751-1781	-0.009 ± 0.013
1783-1813	$+0.0025 \pm 0.010$
1815-1846	-0.0151 ± 0.007
1847-1875	$+0.016 \pm 0.0034$
1877-1912	-0.016 ± 0.0028

Hough * found from heliometer observations of Mars,

1899-1920	$-0.046 \pm 0.007.$
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Dr. Jones † subsequently extended this series to 1924, attaching great weight to the observations near the oppositions of 1920, 1922, 1924, and obtained

1899-1924	$-0.026 \pm 0.0048.$
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Dr. Jones states that he had found a smaller value from the Greenwich meridian observations of Mars from 1899 to 1920, but does not give the figure.

To these may be added Backlund's determination from Encke's comet :

1819-1858	$-0.025.$
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I omit Newcomb's final determinations, because the uneven distribution of weight renders it impossible to compare them with the fluctuations.

To appreciate the significance of these values, it will be convenient to arrange them in chronological order and compare each with the annual change in the amount of the lunar fluctuation. My conclusion is that the mass of Venus varies inversely with the change in the amount of that fluctuation.

Our series begins with an upward slope of the curve of lunar fluctuations. The large negative correction to Leverrier's mass of Venus given by the solar observations appears to be well determined, for there is a conspicuous accord between the different groups. It would probably be better to close this super-group at the year 1778 in view of the inferiority of the solar observations after that date. With this amendment the value given by the solar observations becomes -0.029 ± 0.0034 . I find by extrapolation from Dr. Brown's table, corrected as above, $+7''.73$ for the total fluctuation of the Moon at the beginning of 1750, and by interpolation from my own table $+13''.90$ for the total fluctuation at the end of 1778, a total change of $+0''.21$ per annum.

From 1783 to 1813 the motion of the curve is mainly downwards. The Mars observations give $+0.0025 \pm 0.010$, with an annual change in the lunar fluctuation amounting to $-0''.19$. On the other hand, the solar observations from 1793 to 1818 give -0.042 ± 0.018 , with an annual change

* *Cape Annals*, vol. viii. (1923).

† *M.N.*, 85 (1925), 853.

in the fluctuation amounting to $-0''.27$. In view of their discordance, their probable errors, and the inferior character of the solar observations of this date, which may well attach to the Mars observations as well, I do not propose to use either of these results.

From 1815 to 1846 the downward slope is appreciably less steep. Here the Mars observations give $-0.15 \pm .007$ for an annual change of fluctuation of $-0''.12$. This is a perceptibly larger value than that given by the good solar observations from 1750 to 1778 for a positive change in fluctuation, and it is at least smaller than that which the Mars observations gave for the steeper downward slope of 1783-1813. From 1810 to 1842 the solar observations give $-.000 \pm .012$ for the somewhat steeper downward slope of $-0''.14$ per annum. The true value lies probably between these extremes, but I do not propose to use either determination. The same slope, $-0''.14$ per annum, is found associated with a value of -0.25 for the years 1810-58 in Backlund's determination from Encke's comet. In view of the other determinations on this slope this is probably too small a value. The three determinations seem to suggest that $-.010$ is the approximate correction to the mass of Venus for a downward slope of $0''.14$ per annum. But in view of their epoch I think it unsafe to use the Mars and Sun observations of this date. I retain Backlund's determination with half weight.

From 1843 to 1860 the solar observations give $-.021 \pm .007$, with an annual change of fluctuation amounting to $-0''.20$. If a negative change of fluctuation corresponds to an increase of mass, this is in good agreement with Backlund's value. But the observations of 1819 to 1842 and of 1815 to 1846 would lead us to expect a rather larger value.

After this date the downward slope becomes steeper and the determined mass of Venus definitely larger. From 1847 to 1875 the annual change of fluctuation averages $-0''.41$, and the Mars observations yield a correction of $-0.16 \pm .0034$. This is the largest value in the whole series. For 1864-1900 we have Dr. Cowell's determination of $-.007$ for an average change of fluctuation of $-0''.42$, while for 1867-92 Newcomb's results give $+.006$ for an average change of slope of $-0''.52$. These bring us to the end of the long downward slope shown on the diagram, and it is noteworthy that its steepest part coincides with the largest values for the mass of Venus. In view of the probable errors attaching to these determinations, we cannot expect the observed mass to vary exactly in inverse proportion to the change in fluctuation.

The Mars observations from 1877 to 1912 lie partly on the downward and partly on the upward slope of the curve. They yield a value of $-0.16 \pm .0028$ for a mean annual change of $-0''.05$ in the fluctuation. This value, as will be seen, is intermediate between those found on the steep downward and steep upward slopes of the curve.

Hough's determination from the Mars observations of 1800-1920 is based almost wholly on observations taken on the upward slope, though the curve is flattened at each end. The mean annual change of fluctuation amounts to $+0''.23$, the highest in the whole series, and

the correction to the mass of Venus is the lowest, $-.046 \pm .007$. Dr. Jones, by extending this series to 1924 and attaching special weight to the observations of 1920, 1922, and 1924, increased the correction to $-.026 \pm .0048$. By so doing he carried the series over a curve into a new downward slope, and diminished the annual change of fluctuation to $+0''.14$. It will be observed that the solar series of 1750-78 falls between Hough's and Jones's determinations of the mass of Venus, and also between their mean annual change of fluctuation. It is not surprising that Dr. Jones should have found that the Greenwich observations of Mars of 1899-1920, covering the same range as Hough's, gave a smaller value to the mass of Venus than he himself had determined from the Cape series extended to 1924. Finally, Dr. Jones's determination from the solar observations of 1836-1923, with its moderate mean annual change of $-0''.17$ in the fluctuation, gives an intermediate value of $-.007 \pm .0019$ to the correction to the mass of Venus.

From this discussion it is clear that the observed mass of Venus has been comparatively small when the amount of the lunar fluctuation, and therefore also of the solar fluctuation, has been increasing, while it has been comparatively large when that amount has been diminishing. This has been proved by means of the solar observations for the increase of fluctuation in the latter half of the eighteenth century, from the observations of Mars and the Sun for the diminution during the nineteenth century, and from the observations of Mars for the increase and subsequent diminution in the twentieth century. Observations of the two bodies are in good agreement, and the observations of Encke's comet do not tell against them. It is further clear that, if we have regard to the probable errors attaching to the separate determinations, the observed mass of Venus varies with the degree to which the lunar fluctuation varies. It must be remembered, however, that these probable errors are assigned on no uniform principle, and it is unsafe to weight the different determinations by the probable errors assigned to them.

The mass of Venus at any particular date may be expressed in the form

$$a + \phi b,$$

where a is the mass corresponding to the mean motion of the Moon, expressed as a correction to Leverrier's mass taken as unity, ϕ is the annual increase in seconds in the Moon's fluctuation, and b is a constant.

In forming equations of condition to determine a and b , I have thought it best to weight each determination by the square of the number of years over which it was made, giving half weight to Backlund's determination and to Dr. Jones's determination from the observations of Mars in 1899-1924.* In the latter case I was influenced by the fact that the greater part of these observations were already included in Hough's determination, and by the fact that the observations at the end of the series, which doubtless corresponded to a different mass from the others in the series, had been given a weight by Dr. Jones which must have tended to distort the result.

* But see note at end of paper.

I thus obtain the following equations and residuals :—

Weight.	Date.	a.	b.		Res.
841	1750-1778	1	+0.21	= -0.029	+0.004
800	1819-1858	1	-0.14	-0.025	-0.014
576	1843-1866	1	-0.20	-0.021	-0.014
841	1847-1875	1	-0.41	+0.016	+0.010
1369	1864-1900	1	-0.42	+0.007	0.000
676	1867-1892	1	-0.52	+0.006	-0.007
1296	1877-1912	1	-0.05	-0.016	+0.001
484	1899-1920	1	+0.23	-0.046	-0.011
338	1899-1924	1	+0.14	-0.026	+0.003
7744	1836-1923	1	-0.17	-0.007	+0.002

Eight of the determinations had had probable errors assigned to them. Although they were not weighted by their probable errors, three of the eight give residuals less than their probable error, four others residuals between one and two times the probable error, and one a residual more than double the probable error. Of the two remaining determinations, one was satisfied absolutely. The residuals are, therefore, in good agreement with expectation.

From these equations I obtain the following result :—

$$\text{Mass of Venus} = \text{Leverrier's Mass} - 0.020 \pm 0.0019 - (0.064 \pm 0.0078)\phi.$$

From this it follows that the mean mass of Venus is equal to Leverrier's provisional mass multiplied by 0.980 ± 0.0019 . As Leverrier's mass was $1/401847$, this gives us for the mean mass $1/(410000 \pm 790)$. Taking this mass as standard, we can express the mass at any date in the form

$$\frac{1 - (0.065 \pm 0.0080)\phi}{410000 \pm 790}.*$$

Now it seems impossible to suppose that this fluctuation in the mass of Venus, occurring as it does in the observations of Sun and Mars alike, and keeping pace with the fluctuations in the longitude of the Sun and Moon, can be due to observational error or to the Earth's rotation. The mass of Venus, being determined from its effect on the coefficients of the inequalities produced by the action of Venus on the longitudes of the Sun and Mars, would appear to be practically independent of systematic errors, because no observational errors would be likely to be governed by the same periods as the different Venus terms. There appears, therefore, to be no possible conclusion except that this fluctuation in the mass of Venus is real. By the mass of Venus I mean only its action as a perturbing force on the motion of other planets and, presumably, of their satellites. A fluctuation of mass is contrary to the theory on which celestial mechanics have hitherto been worked out, and in our ignorance of the nature and cause of this change of mass, it seems wisest to postulate no change in theory beyond what the observations attest. Now the mass of Venus as it enters into the perturbations of planetary orbits is the relative mass of Venus

* But see note at end of paper.

as compared with the Sun, and our study does not inform us whether it is the mass of Venus or of the Sun that is fluctuating. There appears, however, to be no reason why the relative mass of Venus should fluctuate, while the relative masses of the other planets remain stable. The fluctuation is observed in Venus only, because no other planet's mass has been determined from a succession of observations spread over the different phases of the fluctuation of the longitudes of Sun and Moon. It seems probable, therefore, that this fluctuation in longitude is accompanied by a fluctuation in the relative masses of the Sun and planets. As the fluctuation in longitude affects Mercury, Venus, the Earth, and the Moon, it appears that Dr. Brown was right when in 1914 he described the fluctuation as a kind of surge spreading through the solar system. But since this fluctuation exhausts so much of the inequalities in the longitudes of the Sun and Moon as are not gravitationally explained, there is no room left for any visible effect of a fluctuation in the Earth's rotation. We may therefore declare with confidence that while the Earth's rotation may be affected by a secular retardation, it is certainly not affected by a fluctuation.

4. *The Equinox*.—Another term which has seemed rather elusive hitherto is the position of the equinox, and I therefore resolved to investigate the possibility of this term being affected by the same fluctuation. Defining the position of the equinox as the correction required in order to reduce the Right Ascensions of Newcomb's system N_1 to the true equinox, and converting time into arc for the sake of uniformity, I extract the following positions, determined from Greenwich meridian observation of the Sun, from the Introduction to the *First Greenwich Catalogue of Stars for 1925.0* :—

1851	0°00	1891	+0°03
1857	−0°29	1901	−0°21
1864	−0°48	1911	−0°68
1872	−0°44	1919	−1°10
1882	−0°53		

Mr. Cullen, in *M.N.*, **85**, 1005, gives the more recent values as follows :—

1897–1900	−0°18	1915–1921	−1°10
1901–1905	−0°26	1922–1924	−0°96
1906–1914	−0°60		

Newcomb, in his *Researches on the Motion of the Moon*, part ii., 212–22, gives the following values from the occultations, where I have computed the probable errors with Newcomb's weights from the residuals of his solution :—

1720	+0°15±0°63	1848	−0°03±0°25
1761	−4°05±1°25	1865	+0°15±0°25
1792	−1°02±0°50	1882	−0°30±0°25
1811	+0°06±0°63	1899	−0°18±0°25
1829	+0°12±0°31		

No value in either series appears to be very exact until correction for variation in latitude was introduced into the reduction of the Greenwich observations in 1897. During the nineteenth century down to that date the position of the equinox appears to have remained close to Newcomb's value, though the Greenwich series shows a negative trend from 1851 to 1864, and after a distinct negative value lasting till 1882 approximately, a positive trend appears. This negative trend may be connected with the minor fluctuation, which between 1840 and 1877 makes the Moon's longitude greater than it should be at the average speed which obtained from 1786 to 1895.

When we come to the accurate observations, beginning in 1897, we find a rapid change of the position of the equinox in a negative direction, far steeper than any change observed during the preceding century. About 1919 this change is arrested, and a small movement in the opposite direction makes its appearance. Now these changes coincide exactly with the changes in the solar and lunar fluctuations. If we assume that Newcomb's equinox represents much the same slope as the fluctuation curve between 1786 and 1895, we should expect a sudden divergence about 1897, and the indication appears to be that a negative movement of the equinox corresponds to a positive movement of the Moon, and *vice versa*. It is noteworthy that the turn about 1919 is also common both to the fluctuations and to the equinox, the renewed downward slope being a little steeper than the mean slope of 1786-1895. For these reasons I hold that we are justified in regarding the motion of the equinox as a result of the same movement which produces fluctuations in the longitudes of the Sun, Moon, and planets, and in the mass of Venus. It means that the cause of this fluctuation operates on the planes of the orbits as well as on the positions of planets in their orbits, and on their masses relative to the Sun.

I have not yet attempted to determine the position and motion of the equinox by least squares,* but I find by trial and error that the following formula should reduce Right Ascensions from Newcomb's equinox to the mean equinox:—

$$\begin{aligned}\Delta RA &= +1''.20 - 2''.46T \quad (1800.0) \\ &= -1''.26 - 2''.46T \quad (1900.0).\end{aligned}$$

To reduce to true equinox, as affected by fluctuation, this formula becomes

$$\Delta RA = -1''.26 - 2''.46T - 0.075m \quad (1900.0),$$

where m is the total fluctuation of the Moon at date.

From this it would appear that the fluctuation of the equinox in relation to the fixed stars is in the same direction as the fluctuation of the Sun's longitude, but amounts to less than three-fourths of the amount of the solar fluctuation.

5. *The Moon's Node*.—If the Earth's longitude and plane of orbit are affected by this cause, and if, as we know, the Moon's longitude is so affected, it seems not unreasonable to inquire whether the Moon's orbital plane is affected. This should reveal itself in the motion of the

* But see note at end of paper.

node. In 1914 Dr. Brown* analysed the Greenwich meridian observations from 1847 to 1901 for the position and motion of the node. The values obtained by him are stated to have been subject to a probable error of or about $\pm 1'' \pm 1''T$. In 1915† he made a small correction to this result, but not so as to affect the probable error. In 1924 Dr. Jones found‡ from the Cape occultations, 1880-1922, a correction to Hansen's position of the node at epoch 1900.0 :

$$\sin i \Delta\Omega = +1''.01 \pm''.053 + (3''.45 \pm''.54)T.$$

Reduced to Brown's position and motion of the node, this becomes

$$\Delta\Omega = +1''.87 \pm''.59 + (26''.57 \pm 6''.03)T.$$

In view of the large error attaching to the centennial term Dr. Jones rejected it, and supposing by oversight that Dr. Brown's mid-epoch had been 1850, 50 years before his own, he adopted twice the correction at epoch as the correction to centennial motion, thus obtaining $+3''.74$ as his definitive correction to the centennial motion of the node. But since Dr. Brown's mid-epoch had been 1874.5, only 25 years before his own, he ought to have doubled this term and to have adopted $+7''.48$.

Now a glance at the diagram will show that Dr. Brown's determination was made over a fairly straight downward slope of the fluctuation curve, rounded a little at each end. If the motion of the node is affected by the system of fluctuations discussed in this paper, there would be very little within that range of years to produce discordances. Hence, perhaps, the small probable error. The range of years examined by Dr. Jones is marked by two strongly pronounced bends of the curve, and should come out with a large probable error if the value is affected by the fluctuations. Its mean direction differs little from the mean motion of the Moon. But Dr. Jones's determinations, classified according to groups of years, seem to show a greater advance in $\sin i \Delta\Omega$ from epoch 1888 to epoch 1901.5, included in Dr. Brown's investigation, than from epoch 1901.5 to epoch 1915 when the upward slope was pronounced. It is clear, therefore, that a part only of this observed change in the motion of the node can be attributed to the fluctuations, and a doubt arises whether it is real at all. Herr Schoch, to whose studies of eclipses reference has been made above, has long desired to make a correction of $+8''T$ to Dr. Brown's longitude of the node in order to satisfy the total eclipses of the Sun of $+1239$ June 3 and $+1715$ May 3. I had always objected to this on the ground of the extreme accuracy of Dr. Brown's determination. The subject is one which demands further investigation.

6. "*Trepidation*."—It is probable that the movement discussed in this paper affects a number of other terms which have yet to be investigated, and it is therefore desirable to have a name for it. It would be cumbersome to use the word "fluctuation," followed by a list of all the terms known or supposed to be affected. The word "surge" used by Dr. Brown in his paper of 1914 is hardly likely to be adopted as a technical

* *M.N.*, 74, 552-61.

† *Ibid.*, 75, 506-8.

‡ *Ibid.*, 85, 25.

term. I therefore propose to use the term "trepidation," which expresses the kind of movement that we have in view, and which has the advantage of being the ancient term for one of the forms in which it manifests itself. The trepidation of the equinoxes, in which some Greek and most mediæval astronomers believed, was a long-period forward and backward motion of the equinox, changing rather by jerks than with the curvature that characterises most changes of celestial motions. Such a motion of the equinox with a major period of 260 years is exactly one of the phenomena under discussion, and I propose therefore to give the name "trepidation" to the whole of this group of phenomena, just as we give the name "precession" not only to the secular motion of the equinox itself, but to all apparent motions arising from the secular changes in the relative positions of the poles of the Earth's rotation and revolution.

One result of this inquiry is that it brings out the accuracy of our observations, especially of the Greenwich observations, and tends to show that much of what was attributed to instrumental errors is really to be assigned to physical causes.

Summary.

1. The observed fluctuations in the longitude of the Sun and Moon are compared in the light of independent determinations of the motions of the two bodies made over the years 1843–1925.

2. It is found that the discordances between theory and observation in the longitude of the Sun can be removed by supposing an acceleration approximately equal to that determined from ancient observations and a fluctuation bearing a constant ratio of 1/9.76 to the Moon's fluctuation. A definitive value for the Sun's longitude is found on this hypothesis.

3. It is found that the mass of Venus and the motion of the equinox are affected by the same fluctuation.

4. A question is raised, but not answered, whether the motion of the Moon's node is so affected.

5. It is concluded that these fluctuations cannot be due to a fluctuation in the Earth's rotation, but must be due to what Dr. Brown termed "a surge spreading through the solar system."

6. It is proposed to call this group of phenomena "trepidation."

University Observatory, Oxford :
1926 December 9.

Note.—While this paper was in proof I noticed that Dr. Brown has not applied to the observed lunar and solar places the correction of $+8.005$ or $+0.075$ necessary to reduce the Greenwich Right Ascensions of 1915 and subsequent years to the equinox of Newcomb's system N_1 .^{*} In the case of the Moon, where each observed error is reduced to longitude, this correction takes the form of $+0.075 \cos \epsilon$. The introduction of

^{*} See the Introduction to the *First 1925 Catalogue* and Mr. Cullen's paper in *M.N.*, **85**, 1000–1006.

this correction modifies slightly the expression for the Sun's longitude. Formula (3) now becomes

$$\Delta L' = -0''.040 - 0''.026T + 0''.867T^2 + 0.1051fm \quad (1884.5) \\ \pm ''\cdot035 \pm ''\cdot12 \pm ''\cdot15 \pm \cdot0075.$$

The reciprocal of the coefficient of fm becomes 9.51 ± 0.68 .

The total correction to Newcomb's longitude of the Sun referred to the equinox of his system N_1 becomes

$$+1''.03 + 3''.81T + 1''.36T^2 \quad (1884.5) \quad . \quad . \quad (4)$$

$$-1''.22 + 1''.51T + 1''.36T^2 \quad (1800.0) \quad . \quad . \quad (5)$$

$$+1''.65 + 4''.23T + 1''.36T^2 \quad (1900.0) \quad . \quad . \quad (6)$$

The expression for the secular acceleration becomes

$$+1''.34 \pm ''\cdot15 + (0.1051 \pm \cdot0075)\Delta s,$$

where Δs represents any correction which it may be found necessary to apply to the adopted value of $+10''.80$ for the total sidereal secular acceleration of the Moon.

It has also occurred to me that I ought to have weighted the determinations of the mass of Venus by the number of years over which each determination was made, not by the square of that number.

Revising the weights accordingly, I obtain

$$\text{Mass of Venus} = \text{Leverrier's Mass} - 0.0182 \pm 0.0019 - (0.0546 \pm 0.0068)\phi.$$

Expressed in terms of the Sun's mass this becomes

$$\frac{1 - (0.0556 \pm 0.0069)\phi}{409300 \pm 790}.$$

For the reduction of Right Ascensions from Newcomb's equinox to the mean equinox I have now obtained by least squares:—

$$\Delta RA = -1''.18 - 2''.31T \quad (1900.0).$$

To reduce to true equinox as affected by trepidation, the formula becomes

$$\Delta RA = -1''.18 - 2''.31T - 0.0655m \quad (1900.0) \\ \pm ''\cdot091 \pm ''\cdot215 \pm 0.0086.$$

I hope to deal further with these quantities in one or more future papers.

University Observatory, Oxford :
1927 January 5.

*Mean Areas and Heliographic Latitudes of Sunspots in the Year 1925.**(Communicated by the Astronomer Royal.)*

The results here given are in continuation of those in the *Monthly Notices*, **85**, 1006, and are derived from the measurement of photographs taken at the Royal Observatories of Greenwich and the Cape, and in India at the Kodaikānal Observatory and at Dehra Dûn. Father Rodés, Director of the Ebro Observatory, Tortosa, Spain, kindly lent for measurement two plates taken on July 23 and November 26 respectively. Photographs on these two days were not available elsewhere.

Table I. gives the mean daily area of umbræ, whole spots, and faculæ for each synodic rotation of the Sun included in the year 1925, whilst the mean values for each year as a whole from 1923 to 1925 are given in Table II.

Table III. gives for each rotation in the year 1925 the mean daily area of the whole spots (corrected for foreshortening) and the mean heliographic latitude of the spotted areas for both hemispheres, northern and southern. The mean heliographic latitude of the entire spotted area and the mean distance from the equator of all spots are also tabulated. The mean values for the years 1923 to 1925 are given in Table IV. Tables II. and IV. are in continuation of similar tables given in the *Monthly Notices*; for the years 1874 to 1888, **49**, 381 and 382; 1889 to 1902, **63**, 465 and 466, 1901 to 1914, **76**, 402 and 403; 1913 to 1924, **85**, 1007 and 1008.

The rotations in Tables I. and III. are numbered in continuation of Carrington's series (*Observations of Solar Spots made at Redhill*, by R. C. Carrington, F.R.S.), No. 1 being the rotation commencing 1853 November 9. The assumed prime meridian is that which passed through the ascending node at mean noon on 1854 January 1, and the assumed period of the Sun's sidereal rotation is 25·38 days.

The principal features of the record for the year 1925 were as follows:—

1. A large increase in the mean daily area of spots—830 units as compared with 276 units for the previous year and 55 units for the minimum year 1923.

2. A great increase in spot activity occurred towards the end of the year. The mean daily area of 3438 units for Rotation 966 is the largest value as yet recorded in the *Greenwich Results* for an individual rotation, the nearest approach being 3089 units for Rotation 854 in 1917, the maximum year of the previous cycle.

3. Of the total spot activity, 62 per cent. was contributed by the northern hemisphere.

4. The amount of faculæ showed a large and steady increase.

5. The mean latitude of all spots was 20° as compared with 23° for the previous year. The latitudes of individual groups ranged from 9° to 38° .

6. There were 19 days without spots as compared with 97 for the year 1924 and 171 for 1923. On no day in 1925 was the Sun's disc free from both spots and faculæ.

7. The number of separate groups lasting for at least two days was 242. Of these 242 groups, 147 were in the northern hemisphere and 95 in the southern. Besides these there were recorded 111 small groups (mostly consisting of one or two very small spots) visible on one day only; 71 were in the northern and 40 in the southern hemisphere.

8. In spite of the rapidly increasing activity of spots and faculæ indicated by the year's results, the mean latitude of the spots indicates that the epoch is between two and three years before the maximum of the cycle.

TABLE I.

No. of Rota- tion.	Rotation commenced G.M.T.	No. of Days on which Sun photo- graphed.	Mean of Daily Areas.						
			Projected.*			Corrected for Fore- shortening.†			
			Umbrae.	Whole Spots.	Faculae.	Umbrae.	Whole Spots.	Faculae.	
	1925.								
954	Jan. 10.30	28	11	59	694	8	47	790	
955	Feb. 6.64	27	74	451	1021	53	321	1111	
956	Mar. 5.98	27	56	345	728	44	274	824	
957	Apr. 2.28	28	72	359	1103	56	288	1291	
958	Apr. 29.55	27	163	985	949	122	732	1129	
959	May 26.77	27	170	964	1338	136	781	1575	
960	June 22.97	27	96	571	1599	70	416	1851	
961	July 20.17	28	82	444	1357	62	353	1589	
962	Aug. 16.39	27	137	709	1618	101	538	1871	
963	Sept. 12.64	27	199	1060	1944	152	833	2286	
964	Oct. 9.92	27	302	1700	2399	223	1298	2756	
965	Nov. 6.22	28	348	2207	2425	255	1614	2816	
966	Dec. 3.53	27	799	4729	2815	580	3438	3285	

TABLE II.

Year.	No. of Days.		Mean Daily Areas.					
	Photo-graphed.	Without Spots.	Projected.*			Corrected for Foreshortening.†		
			Umbrae.	Whole Spots.	Faculae.	Umbrae.	Whole Spots.	Faculae.
1923	365	171	15	74	189	10	55	222
1924	366	97	62	361	498	46	276	575
1925	365	19	189	1102	1508	141	830	1750

* Expressed in millionths of the Sun's disc.

† Expressed in millionths of the Sun's hemisphere.

TABLE III.

No. of Rotation.	Rotation commenced G.M.T.		Spots NORTH of the Equator.		Spots SOUTH of the Equator.		Mean Latitude of entire Spotted Area.	Mean Distance from Equator of all Spots.
			Mean Daily Area.	Mean Heliographic Latitude.	Mean Daily Area.	Mean Heliographic Latitude.		
954	1925 Jan.	10.30	42	22° 64	5	33° 56	+ 16° 29	23° 87
955	Feb.	6.64	65	11° 79	256	20° 19	- 13° 70	18° 49
956	Mar.	5.98	134	20° 56	139	21° 83	- 1° 04	21° 21
957	Apr.	2.28	202	21° 89	86	25° 26	+ 7° 76	22° 90
958	Apr.	29.55	683	19° 30	50	19° 12	+ 16° 69	19° 28
959	May	26.77	601	21° 93	180	21° 62	+ 11° 88	21° 86
960	June	22.97	82	19° 82	334	16° 16	- 9° 10	16° 88
961	July	20.17	247	22° 36	106	27° 50	+ 7° 42	23° 90
962	Aug.	16.39	367	21° 48	171	19° 66	+ 8° 38	20° 90
963	Sept.	12.64	417	19° 07	417	19° 91	- 0° 42	19° 49
964	Oct.	9.92	518	19° 32	780	18° 41	- 3° 34	18° 77
965	Nov.	6.22	1186	19° 40	428	16° 69	+ 9° 83	18° 68
966	Dec.	3.53	2256	21° 87	1182	19° 55	+ 7° 63	21° 07

TABLE IV.

Year.	No. of Days on which Sun photographed.	Spots NORTH of the Equator.		Spots SOUTH of the Equator.		Mean Latitude of entire Spotted Area.	Mean Distance from Equator of all Spots.
		Mean Daily Area.	Mean Heliographic Latitude.	Mean Daily Area.	Mean Heliographic Latitude.		
1923	365	33	13° 88	22	17° 32	+ 1° 34	15° 26
1924	366	232	22° 19	45	25° 54	+ 14° 49	22° 73
1925	365	517	20° 70	313	19° 37	+ 5° 60	20° 20

Royal Observatory, Greenwich :
1926 November 19.

Double Star Measurements, 1922-5. By B. M. Peek, M.A.

The following stars were measured at Boscombe during the years 1922-5 with the 5-inch Watson refractor and the Society's Cooke micrometer. During the latter part of this period measurements were almost entirely confined to stars lying within 35° of the North Pole. As the micrometer has no provision for bright wire illumination, stars below 9th magnitude were only measured with difficulty.

The separations given for $\Sigma 1110$ (Castor) and $\Sigma 1424$ (γ Leo) are slightly larger than those generally accepted, but agree very well with those determined by Rev. T. E. R. Phillips and Mr. F. J. Hargreaves during some experiments conducted with a variable objective grating.

Star Σ .	1900.				Mags.		P.A.	No. of Nights.	Dist.	No. of Nights.	Epoch 1900 +.
	R.A.		Dec.								
	h	m	°	'							
61 (65 Psc.)	0	44	+27	10	6.0	6.0	295.7	3	4.43	4	22.93
202 (α Psc.)	1	57	+ 2	16	2.8	3.9	307.8	4	2.62	4	22.97
349 ..	3	3	+63	28	7.4	8.1	319.5	4	6.02	4	24.15
362 .. (AB)	3	8	+59	40	7.7	8.0	141.4	4	7.18	4	25.17
374 ..	3	15	+67	6	7.0	8.5	295.4	2	10.93	2	25.18
638 ..	5	3	+69	42	7.5	8.5	219.6	2	5.28	2	25.24
946 ..	6	36	+59	36	7.1	9.0	128.8	4	4.19	4	24.27
948 (12 Lyn.) { (AB) } { (AC) }	6	37	+59	32	5.2	6.1	103.7	3	1.74	3	24.29
						7.4	307.5	3	8.49	3	
1062 (19 Lyn.)	7	15	+55	28	5.3	6.6	314.5	3	14.69	3	24.39
1110 (Castor)	7	28	+32	7	2.7	3.7	213.9	3	4.85	4	23.34
							213.0	3	4.70	3	25.37
1424 (γ Leo)	10	15	+20	21	2.0	3.5	116.3	4	3.98	5	23.35
							116.8	3	4.02	3	25.37
1553 ..	11	31	+56	41	7.3	7.8	168.1	4	5.84	3	24.47
1625 ..	12	12	+80	41	6.5	7.0	218.6	3	14.38	3	24.54
1744 (ζ U. Ma.) [*]	13	20	+55	27	2.1	4.2	149.7	2	14.48	2	22.55
1872 ..	14	38	+58	24	7.0	8.0	44.7	5	7.61	4	25.69
1877 (ϵ Boo.)	14	41	+27	30	3.0	6.3	331.3	2	2.78	2	22.59
1882 ..	14	42	+61	31	7.2	8.7	0.2	4	11.45	3	25.74
1888 (ξ Boo.)	14	47	+19	31	4.7	6.6	51.8	5	2.85	5	22.58
							47.3	11	2.92	8	23.58
1909 (44 Boo.)	15	0	+48	3	5.2	6.1	244.8	3	3.59	3	22.75
1972 ..	15	35	+80	47	6.1	7.0	80.6	3	30.92	3	25.70
1965 (ζ Cr. B.)	15	36	+36	58	4.9	5.9	302.7	4	6.03	3	22.74
2092 ..	16	38	+60	53	7.7	8.8	4.6	4	8.07	4	23.83
2087 ..	16	38	+23	50	8.2	8.2	288.2	3	5.36	3	22.62
2130 (μ Dra.)	17	3	+54	36	5.0	5.1	121.6	5	2.29	3	22.78

Star Σ .	1900.				Mags.		P.A.	No. of Nights.	Dist.	No. of Nights.	Epoch 1900 +.
	R.A.		Dec.								
	h	m	°	'							
2179 ..	17	22	+72	41	8.2	8.8	210.5	4	5.33	3	23.94
2199 ..	17	37	+55	48	7.2	7.8	76.8	4	1.73	3	24.4
2241 (ψ -Dra.)	17	44	+72	12	4.0	5.2	15.0	3	30.36	4	23.62
2272 (70 Oph.)	18	1	+ 2	32	4.5	6.0	{ 129.7 129.2	{ 11 16	{ 5.76 5.92	{ 12 14	{ 22.76 23.64
2280 (100 Her.)	18	4	+26	5	5.9	5.9	182.5	3	14.17	3	22.73
2308 (40 and 41 Dra.)	18	8	+79	59	5.4	6.1	233.3	4	19.54	3	23.90
2382 (ϵ^1 Lyr.)	18	41	+39	34	4.6	6.3	6.4	5	3.16	6	22.91
2383 (ϵ^2 Lyr.)	18	41	+39	30	4.9	5.2	116.1	7	2.34	5	22.92
2382A-2383A	..	..	(4.6)	(4.9)			172.6	2	208.03	3	22.9
2398	18	42	+59	28	8.2	8.7	154.1	3	16.94	3	22.80
2452	18	57	+75	39	6.7	7.5	217.6	3	5.69	3	23.84
2486	19	10	+49	40	5.8	6.1	216.8	4	8.93	4	22.89
2583 (π Aql.)	19	44	+11	34	6.1	6.7	112.6	3	1.35	3	23.71
2640	20	4	+63	36	6.8	9.9	19.1	5	5.35	.5	23.68
2658	20	11	+52	49	7.0	9.1	114.8	3	5.11	3	23.62
2671	20	16	+55	5	6.0	7.4	337.3	3	3.23	.3	23.63
2727 (γ Del.)	20	42	+15	46	4.0	5.0	269.9	4	10.51	.5	22.82
2758 (61 Cyg.)	21	2	+38	14	5.3	5.9	132.5	4	24.36	5	22.84
2870	22	5	+60	38	8.2	9.2	270.1	6	5.35	2	23.92
2909 (ζ Aqr.)	22	24	- 0	32	4.0	4.1	300.6	4	2.86	6	22.78

Gorse Cliff School, Herne :
1926 December.

A New Variable Star in Taurus. By A. Stanley Williams.

	h	m	s		°	'	
R.A.=5	47	24		Decl.=+26	16.9	(1855).	
R.A.=5	50	12		Decl.=+26	17.6	(1900).	

The variability of this star was first noticed in 1922 November when comparing two photographs taken with a 4.4-inch portrait lens about twenty years previously. Examination of six plates seemed to show that the period was short and that the star was probably a Cepheid. A few visual observations were made with a 6½-inch reflector during the ensuing few months, and these confirmed the variability of the star and pointed to a very short period. They also showed that the visual range of variation was less than the photographic. Good series of visual observations were obtained with the same telescope during each of the three winter half-years of 1923-4, 1924-5, and 1925-6. Altogether 616 visual observations are available for discussion.

The accompanying little chart (fig. 1) has been prepared in order to facilitate the identification of the comparison stars and the variable, which is not in the B.D. It should be mentioned that the positions of the fainter stars are very rough, as they have been inserted by mere eye-estimation from one of the photographs. Also that many faint stars have been omitted altogether. The place of the variable given at the head of this paper has been derived from measures of a photograph dated 1902 April 6.

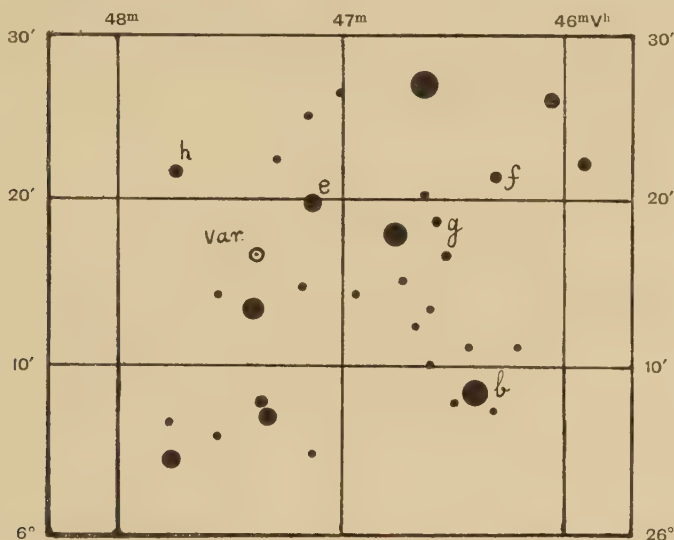


FIG. 1.—Variable Star in Taurus.

In Table I. will be found the positions of the comparison stars used, together with the light-scale, derived from the observations of the years 1922-5, and the magnitudes of the stars on the assumptions that the value of one "step" = 0.05 magnitude and the visual magnitude of the star *g* is 11.55. The assumed magnitude of the last-mentioned star is based on sequence observations made on three nights and extending downwards from the star marked *b* in fig. 1 (B.D. +26° 1001, 8.9 mag.), adopting 9.0 for the visual magnitude of the latter star.

TABLE I.
Comparison Stars.

Star.	Place for 1855.		Scale.	Assumed Mag.
	h m s			
<i>e</i>	R.A. = 5 47 09	Decl. = +26 19.8	23	10.90
<i>h</i>	" = 5 47 45	" = +26 21.9	14	11.35
<i>f</i>	" = 5 46 18	" = +26 21.4	13	11.40
<i>g</i>	" = 5 46 37	" = +26 18.5	10	11.55

The times of 33 maxima have been derived from the observations with the help of a mean light-curve similar to, but not exactly the same as that described later on.* But this light-curve was merely used as a *help* in fixing the times of maximum, and it was ruthlessly disregarded where the observations seemed to require such a course. Particulars of these 33 maxima will be found in Table II. A weight 1 in the fifth column of this table signifies that the maximum was not observed during both the increasing and decreasing light phases, but only during one of them. The time of maximum is therefore not so accurately determined as it is in those cases where a weight of 2 or 3 has been assigned. In these, the star was observed more or less well during both the increasing and decreasing phases.

TABLE II.

Observed and Computed Maxima.

No.	E.	Observed Max. (Helioc.)		Wt.	Comp. Max.	O—C.	O—C ₂ .
		Date.	J.D. 242.				
1	—1038	1923 Nov. 14	3738.469	3	.465	+004	
2	—996	„ 29	3753.426	2	.400	+026	
3	—830	1924 Jan. 27	3812.494	3	.433	+061	
4	—816	Feb. 1	3817.381	2	.411	—030	
5	—813	„ 2	3818.431	1	.478	—047	
6	—712	Mar. 9	3854.408	3	.395	+013	
7	—709	„ 10	3855.442	3	.462	—020	
8	0	Nov. 17	4107.591	3	.593	—002	
9	165	1925 Jan. 15	4166.301	2	.269	+032	
10	202	„ 28	4179.383	2	.427	—044	
11	247	Feb. 13	4195.366	3	.430	—064	
12	312	Mar. 8	4218.439	2	.545	—106	
13	1012	Nov. 12	4467.480	3	.475	+005	—015
14	1032	„ 20	4474.592	3	.588	+004	—014
15	1043	„ 24	4478.539	3	.499	+040	+022
16	1085	Dec. 8	4493.453	3	.435	+018	+003
17	1088	„ 10	4494.519	2	.502	+017	+002
18	1096	„ 12	4497.360	2	.347	+013	—001
19	1102	„ 14	4499.489	3	.481	+008	—005
20	1127	„ 23	4508.386	3	.371	+015	+003
21	1166	1926 Jan. 6	4522.269	2	.240	+029	+019
22	1169	„ 7	4523.317	2	.307	+010	+001
23	1183	„ 12	4528.292	3	.285	+007	—002
24	1186	„ 13	4529.355	3	.352	+003	—006
25	1203	„ 19	4535.394	3	.398	—004	—011

* The maximum is not double in this curve, but only single.

TABLE II.—*continued.*

No.	E.	Observed Max. (Helioc.)		Wt.	Comp. Max.	O—C.	O—C ₂ .
		Date.	J.D. 242.				
26	1214	1926 Jan. 23	4539.329	1	.310	+0.019	+0.013
27	1234	„ 30	4546.449	2	.422	+0.027	+0.022
28	1242	Feb. 2	4549.283	1	.267	+0.016	+0.012
29	1245	„ 3	4550.340	3	.334	+0.006	+0.002
30	1307	„ 25	4572.403	2	.382	+0.021	+0.021
31	1324	Mar. 3	4578.420	2	.427	—0.007	—0.007
32	1366	„ 18	4593.353	2	.363	—0.010	—0.007
33	1428	Apr. 9	4615.392	3	.411	—0.018	—0.012

Notes on the Foregoing Maxima.

No. 3. The rise to maximum was well observed and the time could not be made much earlier. No. 5. A bad night, much mist about. No. 9. A bad night. If two poor observations, made apparently during the rise, had been affected by haze, the time of maximum could be put earlier without doing violence to the other observations. No. 15. There is a distinct double maximum. No. 27. The increase in brightness was unusually slow.

The sixth column of Table II. gives the computed times of maximum (decimals of a day) according to the following elements:—

$$\text{Heliocentric Maximum} = \text{J.D. } 2424107.593 + 0^{\text{d}}.355615 \text{ E,}$$

and the penultimate column the residuals O—C.

The above elements imply a period of uniform length, but inspection of the residuals will show that the observations cannot be satisfied by a period of uniform length. This fact will appear more clearly from the following figures, which give the mean maxima for the three series of observations, with, in the last column, the period derived from the observations of each series. The weights are merely estimates of the relative degree of accuracy of the three series of observations.

Series.	Mean Max.	Wt.	O—C. d	Period. d
1923-4	J.D. 3802.128	1	+0.0078	0.355560
1924-5	4173.341	1	—0.0413	0.355133
1925-6	4528.294	3	+0.0078	0.355549

Evidently there is a long-period irregularity, and the residuals even now could easily be very much reduced by the addition of a suitable sine term to the elements of variation. But it is not absolutely certain that a regular sine term is really indicated by the observations. It seems quite possible that there may have been some breaks or “discontinuities,” to adopt a term first, it is believed, employed by Professor Turner when dealing with the irregularities in the periods of long-period variables.*

* A somewhat similar break was found by Turner when discussing Barnard's observations of an antalgol variable in one of the star clusters.

The amplitude of this long-period irregularity exceeds an hour on a period of $8^h 32^m$, perhaps considerably more than an hour, and the length of its period, if indeed it is strictly periodic, may be two or three years or more.

But in addition to this long-period irregularity, the observations show clearly and definitely the existence of a short-period irregularity which is evidently of a periodic character. This will be seen more clearly from the last column of Table II. headed O-C₂, giving the residuals computed with a simple period of 0^d.35555, practically that derived directly from the observations of 1925-6. These residuals indicate the presence of a term of the nature of a sine term, and one having a period of 25 days (or 70 periods of the main variation), and an amplitude (total range) of 43 minutes would very much reduce the magnitude of the residuals.

Here the matter will be left at present, but the observations of this somewhat interesting variable are being continued with the view of obtaining accurate values for these two irregularities, and in particular of elucidating the real nature and amplitude of the long-period one.

Intimately connected with the foregoing is the question of the influence, if any, that the short-period irregularity may have had on the form of the light-curve, and *vice versa*. These questions will be considered in connection with the mean light-curve.

The mean light-curve has been derived from the observations of 1925-6, the existence of the long-period irregularity in the length of the main period of variation rendering it inadvisable to attempt to combine with them those of the two earlier series of observations. After rejecting some observations that may have been affected by cloud, haze, or mist, there remained 254 more or less good observations. The times of all those made near a maximum, when the star's light was changing most rapidly, were reduced to the sun. These 254 good observations were then arranged in order of phase, and formed into 25 groups of 10 or 11 observations to a group, and the means taken. The period used in this work was 0^d.35555, practically the same as that derived directly from the observations of 1925-6, with an assumed epoch of maximum J.D. 2424467.479. Table III. gives in the first two columns the phase, expressed in hours and minutes and the decimal of a day respectively, from the assumed epoch, and in the third column the mean brightness of the variable in "steps" of the light-scale of Table I. The results are also shown in diagram form in fig. 2, where a mean curve has been carefully drawn through the dots representing the observations. It will be seen that the general form of the curve is that of the extreme Cepheid type known as antalgol. But there is one interesting peculiarity. The curve shows a distinct double maximum. This would seem to be quite an abnormal feature in a star with so short a period as $8^h 32^m$, since it has been stated quite recently by Dr. P. ten Bruggencate that "there exists no short-period Cepheid variable ($P < 1^d$) with a secondary maximum in the light-curve."* The present variable is therefore an exception to this rule, since, with a period of only $8^h 32^m$, the curve shows a distinct secondary maximum about

* *Monthly Notices*, 86, 335.

TABLE III.

Mean Light-curve.

Phase.			Bright- ness. s	Phase.			Bright- ness. s
h	m	d		h	m	d	
0	02	0.0017	16.29	2	50	0.1178	13.80
0	07	0.0052	15.91	3	55	0.1634	13.46
0	12	0.0086	16.62	5	47	0.2410	12.52
0	18	0.0127	17.16	6	49	0.2840	11.87
0	24	0.0169	17.16	7	19	0.3050	11.67
0	30	0.0206	16.91	7	38	0.3179	12.44
0	38	0.0269	17.04	7	51	0.3271	12.12
0	44	0.0308	17.11	8	01	0.3337	12.87
0	52	0.0360	16.69	8	08	0.3391	13.65
I	01	0.0425	16.12	8	16	0.3447	13.35
I	11	0.0496	15.82	8	22	0.3484	14.48
I	26	0.5997	14.80	8	28	0.3524	14.48
I	48	0.0752	15.17				

half an hour after the first or principal one. Moreover, the amplitude of the secondary maximum is but little inferior to that of the first.

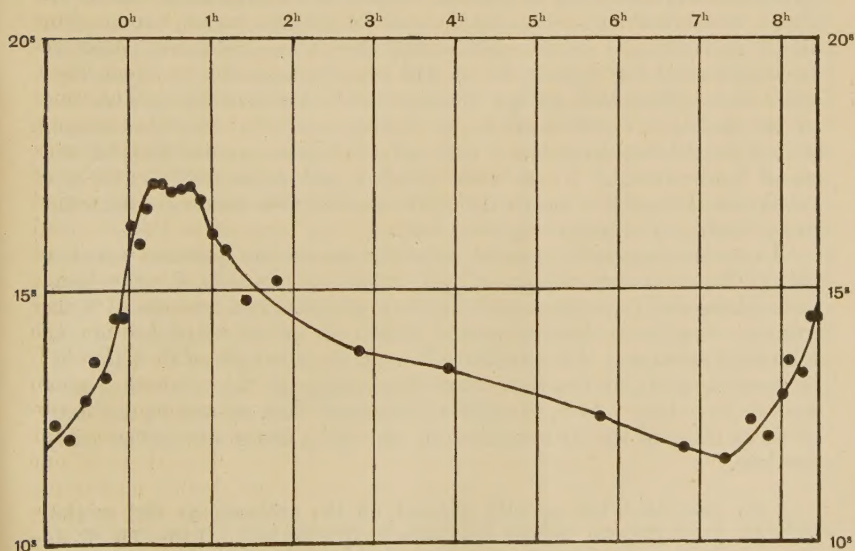


FIG. 2.—Mean Light-curve (1925-6).

There are also indications in the curve of a slight hump about $1\frac{1}{2}$ hours after the first maximum, but the observations are not sufficiently numerous about here for certainty.

Could the double maximum have been due to the existence of the short-period irregularity? This is a question that naturally arises, since the resulting shift in the position of the maximum and other phases must certainly have had some influence on the form of the mean light-curve. But this question must be answered in the negative, because the individual observations of several of the best observed maxima, when plotted upon squared paper, fully confirm the existence of the double maximum. For example, the maxima Nos. 1, 15, 17, 25, and 29 all show a distinct double maximum. Again, might not the double maximum be due to bias on the part of the observer, or, in other words, subjective? Against this explanation it can be said that at the time of the observations the observer had no leaning or predilection in favour of a double maximum. On the contrary, all his efforts were concentrated on observing a single, sharply accentuated maximum, with the special object of fixing the time of maximum, and so the period of variation, as exactly as possible. Consequently any tendency in favour of a double maximum must have been actually forced upon him against this bias for observing a single sharp maximum. It must therefore be concluded that this double maximum had an actual objective existence, and was not due to the presence of the short-period irregularity in the main period of variation.

In some cases the observations do show a single, sharply accentuated maximum, whilst in others the secondary maximum is only feebly indicated. Whilst such differences might not unreasonably be attributed to the leaning or bias on the part of the observer in favour of a single maximum, it is more likely that they were due to real changes in the form of the light-curve in different cycles, since it now seems quite certain that real changes do occur in the forms of the light-curves of the antalgol variables.* It is obvious, however, that the short-period irregularity must have had some influence on the form of the mean light-curve of the present variable, and some part of the discordances of the observations during the rise from minimum to maximum are probably attributable to this cause.

In conclusion, there remains to be considered the question whether the double maximum might not in its turn be responsible for the short-period irregularity in the length of the main period of variation. This question can be at once answered definitely in the negative by the following example. In maximum No. 15 the time given in Table II. is certainly that of the first maximum, although this time is nearly half an hour late. Also, the times of maxima Nos. 17 and 29 are both of them those of the first maximum, and these times are neither early nor late.

* Dr. Henroteau has recently summed up the evidence on the subject. Referring to the antalgol variable XZ Cygni, he remarks that "it has been shown that the shape of the curve changes from cycle to cycle. Shapley found this to be the case for RR Lyræ, Blazko in Moscow proved it for RW Draconis, XZ Cygni, and other stars" (*Popular Astronomy*, 34, 495). There must be other instances where a changeable light-curve is known, or at any rate has been suspected. As long ago as 1902 the writer called attention to the fact that some of the maxima of the antalgol variable Y Lyræ seemed to have been less acute than others (*Monthly Notices*, 62, 204).

Summary.

1. The variable is a Cepheid of the antalgol type with a period of $8^h 32^m$.
2. Maximum magnitude = 11.185.
3. Minimum magnitude = 11.470.
4. Simple elements of variation are—

Heliocentric Maximum = J.D. 2424107.593 + $0^d.355615$ E.

5. There is a long-period irregularity in the main period of variation with an amplitude of over an hour and a period of perhaps several years.

6. There is also a short-period irregularity with an amplitude of 43^m and a period of 25 days.

7. The mean light-curve shows a double maximum which cannot be due to the short-period irregularity in the main period of variation.

8. There are real changes in the form of the light-curve about the time of maximum in different cycles.

Polwarth, St. Mawes :

1926 November 19.

Note added 1926 December 4.—Reference should be made to several important papers published in the *Astronomische Nachrichten*, which have only lately come to the writer's knowledge. In No. 5457 of this publication it is shown by N. Ivanov of Moscow that the light-curve of the antalgol variable AA Aquilæ is clearly changeable, and it even appears possible that some part of the changes observed may have been of a periodical character. In No. 5462 are three papers, one by J. Hellerich on SW Andromedæ, and the other two by S. Blazko on Z Canum Venaticorum and RX Canum Ven. All three of these antalgol stars are shown to possess periodical or perhaps irregular changes in their main periods of variation. Finally No. 5433 contains an important paper by J. Hellerich on RR Lyræ, from which it appears that there was one very remarkable breach or "discontinuity," the observations both before and after the break being satisfied closely by uniform periods. The facts now known concerning the short-period antalgol Cepheids point, it is suggested, to the conclusion that these variables are in a state of at least partial instability.

